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Roope Uusitalo

Essays in Economics of Education

Research Reports
Kansantaloustieteen laitoksen tutkimuksia 79:1999
Dissertationes Oeconomicae
ISBN 951 – 45 – 8705 – 9 (PDF version)

Foreword

Education as a way of increasing human capital is considered to be a basic factor in the growth process of the aggregate economy. The returns to investment into human capital are thus an important issue to analyze. In his Ph.D thesis Mr. Roope Uusitalo studies the effects of education on earnings in Finland. Using a unique individual level data set for men that also includes ability measures and information on family background and appropriate estimation techniques Uusitalo presents new estimates for the return of education in Finland, which are much higher than suggested by earlier studies. Uusitalo also takes a broader issue by trying to explain changes in earnings distribution. He augments a well-known single-index model of skills with the the supply of skills and is able to account for a substantial portion of change in earnings inequality between groups over the 1980s by changes in the supply of skills.

This study is part of the research agenda carried out by the Research Unit on Economic Structures and Growth (RUESG). The aim of RUESG is to conduct theoretical and empirical research into important issues affecting the growth and dynamics of the macroeconomy, the financial system, foreign trade and exchange rates, as well as problems of taxation and econometrics.

RUESG was established in the beginning of 1995 as one of the national centers of excellence selected by the Academy of Finland. It is funded jointly by the Academy of Finland, the University of Helsinki and the Yrjö Jahnsson Foundation. This support is gratefully acknowledged.

Helsinki 30.12. 1998

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Professor of Economics
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Acknowledgments

There are two great parts in a research project. The first is getting all exited about new ideas and the possibilities that a new approach would offer. The second is when the paper is finally done and can be put aside. It is the part in the middle that I had troubles with. Endless efforts trying to make sense of the data and writing the text over and over. Therefore, having finished this thesis, I would like to especially thank all those that helped me with this middle part.

This thesis was written while I worked at the Research Unit on Economic Structures and Growth at the Department of Economics at University of Helsinki. I am most grateful to my colleagues for many fruitful discussions and to the directors of the unit, professors Seppo Honkapohja and Erkki Koskela, for their support. As a part of the program I also got a chance to spend an academic year at Princeton University. I would like to thank great economists and wonderful characters Alan Krueger, Orley Ashenfelter, Henry Farber, David Card and Bo Honore for their insight and suggestions that not only helped solving contemporary problems with this thesis, but also taught me a lot about how economic research really should be done. At Princeton I also wrote the third chapter of this thesis together with Karen Conneely.

There are several others that played an important role in this project. My interest in the economics of education originates to the research that I did while working at the Research Unit on Sociology of Education at the University of Turku, and to the discussions with professors Matti Viren and Osmo Kivinen. Rita Asplund and Reija Lilja examined an earlier version of the first essay and provided useful comments in the early stages of this project. Niels Westergård-Nielsen invited me to spend a few months at Center of Labour Market and Social Research at Århus, where I finished the final chapters. Tor Eriksson, Axel Werwalz, Joop Hartog, Guido Imbens and Gordon Dahl among many others have commented parts of the thesis. Markus Jäntti and Per-Anders Edin examined the final manuscript and made several suggestions that improved the thesis. Without the help from Juhani Sinivuo at Finnish Defense Forces Education Development Center, I would have not had the data that are used in three of the four essays. Several people at Statistics Finland helped making that data useful and answered my strange questions.

The Academy of Finland, the Yrjö Jahnsson Foundation, ASLA-Fulbright, the Finnish Work Environment Fund and the Nordic Research Academy provided financial support at various stages of this project. This support is gratefully acknowledged.

Finally, I would like to thank my friends and family and, especially, my wife Miia for making the life worth living during these long years that I spent working on this dissertation.

Helsinki, December 1998

Roope Uusitalo

Contents

Chapter 1

Introduction

Some forty years after the birth of the human capital theory, education is still one of the central topics in the public policy debate. This is particularly true in Finland which has one of the most expensive education systems in the world. The need to decrease public spending causes pressure to cut the resources that the society allocates to running the school system. On the other hand, it is widely realized that an increasingly complex society and rapid technical change requires highly educated workforce, if the country wishes to succeed in the international competition. Interestingly enough, most of the arguments in this debate are cast in economic terms.

The basic principle of the human capital theory that stresses the role of education as a productivity enhancing investment (Becker 1964) is widely accepted in this discussion. Education policy is directed to meet the skill needs of the modern workplace and to improve the performance of the individuals in the labor market. In fact, education is seen almost as a universal cure to some of the most severe economic problems such as unemployment and poverty. Human capital is also a regarded as key factor in generating higher productivity and economic growth (e.g. Barro and Sala-i-Martin, 1995).

This thesis focuses on the effect of education on individual earnings. This does not necessarily fall far from measuring its effects on productivity. Only few datasets contain better measures of the productivity of individuals. On the other hand, earnings differences are an important outcome themselves. Developments in inequality and poverty have become increasingly important topics and, after recent developments in US and UK, also attracted more and more attention in academic research.

A central theme in this thesis is, how can causal inferences be drawn when only observational data are available. In the natural sciences, causal relationships can be identified using carefully designed controlled experiments. To a limited extent, this is also possible in the social sciences, but education is far outside the scope for technically feasible and morally acceptable experiments. The only option is to use experiments that are set up by nature.

Nature allocates people with different amounts of talent and opportunities. Nature has no need to be fair. Using such natural experiments and economic theory, some inferences on the causal relationships can be drawn.

The approach in this thesis is both structural and parametric. Economic theory is used to formulate the models and, in some cases, to provide empirically testable hypotheses. However, the emphasis is clearly on the empirical work. A lot of effort has been devoted to stretching the statistical methods so that various parameters could be consistently estimated.

This thesis consists of four essays, one of which is joint work with Karen Conneely at Princeton University. All the essays are written to be read by themselves. Therefore, some degree of overlap and repetition is unavoidable. In the following, I briefly introduce the topics of each and summarize their main findings.

Return to education in Finland

The first essay is a straightforward attempt to estimate the rate of return to the years of education in Finland. The major issues are potential biases in the estimates caused by measurement errors in education, ability bias and the endogeneity of educational choice. These problems are tackled by controlling for individual ability differences using data from the Finnish Army psychological tests, and by applying the instrumental variable method in the estimation.

The approach in the first essay is in line with traditional mainstream empirical human capital research. The central issues were discussed already by Griliches (1977). Willis (1985) provides a survey of earlier studies and Card (1994) of more recent studies. Earlier studies relied heavily on test scores in an attempt to remove ability bias from the return to schooling estimates. Generally, it was found that failing to account for the (pre-school) ability differences leads to an overestimate of the return to schooling. This conclusion was largely refuted by a number of studies in the 1990's that relied on various natural experiments and instrumental variable techniques. The instrumental variable estimates were systematically, though often insignificantly, higher than comparable ordinary least squares estimates. Until just a few years ago the empirical evidence was limited to the US data. During last few years several studies have appeared in the UK (Harmon and Walker 1995; Dearden 1995), Sweden (Meghir and Palme 1997), Australia (Miller, Mulvey and Martin 1995) and Netherlands

(Levin 1997). The results in these studies were quite similar to the US findings. This thesis adds one more piece to this accumulating international evidence.

The empirical estimates show that, accounting for measurement error, endogeneity and ability differences, the estimates for the return to additional years of schooling are between 11 and 13%. These are significantly higher figures than earlier estimates from Finnish data (e.g. Asplund 1993). The chapter concludes that the positive ability bias in the ordinary least squares estimates is more than offset by a negative bias caused by endogeneity or measurement error.

Estimating heterogeneous treatment effects in the Becker schooling model[1]

The second and third essays are more focused on statistical issues. In the second essay we take seriously the Becker schooling model, which states that people decide on the schooling investments based on the marginal costs and marginal benefits of education. We note that if the marginal returns vary across individuals, there is no single parameter for the return to schooling. Instead, the appropriate model is a variant of a random coefficients model. The estimation problem is further complicated by the correlation of this random coefficient and the endogenous schooling variable. However, we show that the average return to schooling can still be consistently estimated with traditional instrumental variable method. We also provide maximum likelihood estimates on the extent of unobserved and observed variation in the returns to schooling across individuals.

The implications of variation in program effects are dealt with in the recent "treatment effects" literature. Angrist and Imbens (1995) demonstrate that the instrumental variable method can be used to calculate average causal effects of the treatment. Imbens and Angrist (1994) show that instrumental variables estimates identify "local average treatment effects". Card (1994) discusses these issues less formally in the context of estimating returns to schooling. Heckman (1995, 1997) shows that the conclusions on the consistency of instrumental variables estimates are only valid if the program effects do not vary across individuals or if the variation in program effects does not influence the program participation. Heckman's arguments concern the effect of dichotomous treatment variable. In our essay we show that in a continuous case discussed by Garen (1984) there are some restrictive, but not

[1] Joint work with Karen Conneely

unreasonable assumptions, under which the instrumental variables estimates are still consistent. As empirical evidence we compare instrumental variables estimates to the control function estimates proposed by Heckman and note that the results are close to identical.

Schooling choices and return to skills

The third essay casts some of the issues treated in the first two essays in a discrete choice framework. Eventual education level is determined by a sequence of discrete choices. This essay is an attempt to model these choices and the implications of the choice mechanism on the conditional earnings distributions in the different education levels. The choices among several potentially correlated alternatives are modeled using an ordered generalized extreme value model and predicted outcomes in different education levels are calculated. A dataset that includes measures of various personality traits is used to examine whether rewards for skills vary by the education level and whether this leads to the choices being determined according to comparative advantage.

The econometric methodology in this essay is based on work on selectivity issues in the polychotomous choice models by Lee (1982, 1983, 1995). The Lee approach has been criticized for its restrictive assumptions on the correlation pattern of the unobservable components (Small, 1987, 1994; Schmertmann, 1994; Vella and Gregory, 1996). In this essay some of these assumptions are relaxed. However, it is shown that, a multinomial logit model used by Lee is a reasonable approximation for the data generating process.

Another issue that has caused a major controversy in public press as well as in academic community is the effect of cognitive skills on the success in later life. This debate started from publication of ”The Bell Curve” by Herrnstein and Murray (1994). Though the methodology and the conclusions of the book have been strongly rejected by later research, the debate has launched what could be called a new research program (e.g. Ashenfelter and Rouse 1995; Cawley, Heckman and Lytchacil, 1998). Most of this research avoids biological arguments on heriditance of personality traits but concentrates on the labor market effects of some measurable skills. Understandably, useful data are hard to find and most of the existing research in the U.S. utilizes cognitive skill measures available in National Longitudinal Survey of Youth. My essay provides more empirical evidence to this discussion by using a wide range of personality test scores that were available in the Finnish Army databases. In addition, the essay takes the discussion on the effects of cognitive skills back to the context of the original Roy model (Roy 1951) where individuals choose their careers based on their skill endowments and the returns to these skills in the different sectors.

The empirical results show that several dimensions of skill have significant effects on schooling choices and earnings. However, the effects on earnings are quantitatively small; even detailed information on ability and personality factors explains only a small fraction of earnings variation at a given level of schooling.

Trends in between- and within-group earnings inequality in Finland

The fourth essay deals with the changes in earnings inequality. Inequality has become a very active research area during the 1990's. The increase in research activity has largely been the economic profession's response to the increase in earnings differences in the U.S. over the 1980's. This observation required an explanation. Some of the most successful explanations argued in terms of changes in unionization, opening of international trade, changes in the supply of skilled labor, and the requirements of advanced technology (Levy and Murnane 1992). Of these, only the technology explanation seems to fit the facts. Changes in the technology in the 1980’s appear to have been skill-biased, favoring workers who posses resources and skills to take an advantage of the technological developments.

This essay focuses on one of the more difficult puzzles of the development. A large fraction of the change in the earnings dispersion has occurred between observationally identical workers. A starting point for the explanation is the single-skill model (Card and Lemieux, 1996). In the single-skill model a fraction of the dispersion of earnings within a group of workers with similar education and experience is caused by unobserved differences in ability. A technological change that favors the high-ability workers is then expected to increase the productivity differences both between workers in the different skill groups and increase the dispersion within each group. In the essay, I extend the single-skill model by introducing imperfect substitutability between workers in different skill groups. This creates a role for changes in the relative supply of workers. With this simple extension, the changes in inequality can be analyzed in a familiar supply-demand framework.

Empirical evidence suggests that this extension aids understanding the changes that occurred in the Finnish income distribution over the 1980's. The rapidly increasing supply of educated workers seems to have prevented the increase in earnings inequality that occurred in several other countries. On the other hand, the model does not fully explain the changes in the within-group distribution. The paper provides some evidence that changes in institutional setting, in particular changes in the degree of centralization in wage bargaining, may be responsible for these changes.

Data for the three first essays are created by merging information from the databases of the Finish Army with longitudinal census data. The sample for the first essay is drawn from the men who were in the army in 1970. The second and third essay use a much larger sample of men who were performing their military service in 1982. The army performs various ability and personality tests for all recruits. Since military service is compulsory test scores are available for the majority of the male population. Therefore, labor market effects of individual characteristics can be analyzed using much larger samples than in previous studies.

The army data is then matched with census files using social security numbers that were available in conscription records. Merging data from the army sample required a dataset that contained the whole population. The census data was the only possibility and, although lacking some desirable information, the data were sufficiently rich for the analyses performed. In addition to a large sample size, the Finnish census data have several appealing features. Since most information is based on registers and direct reports from, for example, tax authorities, data is free from recall errors that are common in survey data. Reliability of not only earnings, but also, for example, schooling information is likely to be higher than in most commonly used datasets. Also attrition from the sample is very small.

The fourth essay utilizes microdata from the Income Distribution Surveys (IDS). Designed for this purpose, the IDS data are the best available source for income distribution studies. IDS contains a random representative sample from the population. Although the main income concept is disposable income of the household, detailed information on the market income of individuals is also available. These data also contain information on an important group for which data were not available in army databases, namely the women.

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Chapter 2

Return to Education in Finland[1]

Abstract

This study presents estimates of the return to education in Finland using an individual-level data set that also includes ability measures and information on family background.

It is found that ability test scores have a strong effect on the choice of education and on subsequent earnings. Estimating the return to education with no information on ability leads to an upward bias in the estimates. However, this bias is more than offset by a downward bias caused by endogeneity or measurement error. Instrumental variables estimates that utilize family background variables as instruments produce estimates of the return to schooling that are approximately 60% higher than the least squares estimates.

Keywords: return to education, ability bias, selectivity.

JEL Classification: J24.

2.1 Introduction

In this paper I report evidence on the returns to schooling that exploits a unique data set containing ability test scores from the Finnish army. Since military service is compulsory in Finland and all the men are tested at the beginning of their service, it is possible to construct a linked data set that includes test scores from military service records, income data from tax authorities and information on schooling and family background from Finnish Census. Using these data, I estimate returns to schooling in Finland using test scores as independent variables and using family background as an instrumental variable to correct for measurement error and / or endogeneity in school choices.

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[1] A shorter version of this chapter is forthcoming in Labour Economics

Despite a long debate in the empirical literature on earnings determination, a consensus on the direction and size of the bias in the simple ordinary least squares (OLS) estimates of returns to schooling has yet to appear. Ability differences between individuals with differing amounts of education may bias estimates upward. Alternatively, a number of recent studies suggest that the OLS estimates are more likely to be biased downward. Resolving this issue conclusively would require a series of controlled experiments with random assignments of educational levels.

The majority of the earlier literature on the return to schooling was concerned with the potential omitted variable bias caused by the correlation of unobserved individual abilities with both schooling and earnings. The simplest way to correct for this ability bias appeared to be to obtain a good measure of ability and to include it in the estimated earnings function. Typically, the data sets used for studying the effect of ability bias were constructed using samples that included data on various ability tests taken during military service (Taubman and Wales, 1973). More recent evidence is almost entirely based on a few large scale longitudinal surveys, especially the National Longitudinal Survey of Youth (NLSY), initially surveyed in 1979 (e.g. Blackburn and Neumark 1993, 1995). Including ability measures in earnings equations decreases the schooling coefficients in all these studies.

Other recent approaches for correcting potential biases in the return to education estimates include estimating earnings functions from differences within twins or siblings (Ashenfelter and Krueger 1994; Miller, Mulvey and Martin 1995) and resorting to various "natural experiments" that exploit exogenous sources of variation in schooling (Angrist and Krueger 1991, 1992; Card 1993; Butcher and Case 1994; Harmon and Walker 1995). All these studies conclude that the OLS estimates of the return to education are likely to be biased downward. Corrected estimates range from only slightly above OLS estimates (Angrist and Krueger 1991, 1992) to more than double the OLS estimates. (Harmon and Walker 1995). It is apparent that the two different approaches used in the literature lead to different conclusions.

In this paper I follow the tradition in Griliches (1977) and include various ability measures in earnings equations, but I also treat education as endogenously determined or measured with error, and use information on family background as instrumental variables for education. Thus, I take advantage of the available information on ability of a large sample as in earlier literature, but I also follow the more recent literature in attempting to provide a credible estimate of tha causal effect of schooling on earnings.

My analysis is based on a randomly selected sample of 2,000 men who took the Finnish army ability test in 1970. By combining army test scores, administrative records and a longitudinal data set from Finnish population censuses, I constructed a new panel data set that includes ability measures and information on education and earnings as well as other control variables.

Compared to commonly used large scale survey data sets such as the NLSY, constructing the new data set was very inexpensive. Despite its low cost, the data contain comparable measures of cognitive ability, together with information on schooling and earnings. Since this information is based on administrative records from schools and tax authorities, it is likely to be at least as reliable as self-reported information. The Finnish longitudinal census data file contains information collected every five years (1970, -75, -80, -85 and -90), and it covers a longer time span than, for example, the NLSY. It seems likely that the data construction methods used in this paper may well be applicable also in other countries where schooling and military records may easily be linked together.

The data used in this paper is described in section 2.2. Section 2.3 presents the basic ordinary least squares estimates after controlling for measured ability differences. In section 2.4 the differences in family background are used as an exogenous source of variation in education to create instruments for schooling and to provide estimates free of measurement error / endogeneity bias. Section 2.5 summarizes with a short discussion of why IV and OLS estimates differ.

2.2 Data

The ability test scores used in this study were obtained from the Finnish Defense Forces Basic Ability Test (Peruskoe 1) developed by the Finnish Defense Forces Education Development Center. The test has been administered in unchanged format from 1955 to 1980 for all new recruits at the beginning of their service. In 1981 the ability test was revised and complemented with a broader personality test. Only the ability test is used here. Since military service is compulsory in Finland, the tested group contains almost the entire male cohort.[2]

The ability test consists of three subtests measuring verbal ability, analytical reasoning and mathematical reasoning. Each subtest has 40 multiple choice questions that become gradually more difficult. The measure of verbal ability consists of three types of questions: the examinee has to choose which word is a synonym or antonym of a given word, choose which word pair displays a similar relationship to a given word pair and choose which word does not belong to a given group of words. In the analytical reasoning section, the test-taker is given a matrix of figures arranged according to a certain rule, but with one figure missing. The examinee has to decide which figure completes the matrix. Finally, the mathematical reasoning section consists of simple arithmetic operations, short problems given in a verbal form, and completing number series arranged according to a certain rule.

The scores from different parts are combined and scaled in a range from 1 to 9. This combined score is used as a minimum qualification in the selection of the rookies that are given officer training. Typically a minimum score requirement for selection to the noncommissioned officers' school (RAUK) is 4 and for selection to the reserve officers' school (RUK) minimum is 6.

The selected sample consists of a random sample of 2,000 recruits[3], who had taken the Basic Ability Test in 1970, from the files of the Finnish Defense Forces Education Development Center. Conscription records were then used to match the names to the social security numbers. Finally, the sample was connected to a longitudinal data set of Finnish population censuses.

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[2] A system where every applicant is accepted for alternative (nonmilitary) service was adopted in 1987. Prior to that applications were examined by military authorities and the National Examination Board. Less than 3% of the age group were exempted from military service due to religious or ethical conviction. In addition, approximately 10% were disqualified for health reasons. (Scheinin 1987)

[3] The sample size was limited by the difficulty of collecting the ability test scores. The scores are stored on microfilm and had to be gathered manually. Further difficulties arose because in 1970, the army did not use social security numbers but only names (in some cases only last names and first initials). Since 1982, test scores are electronically stored in a database with proper identification. In fact, a larger sample of approximately 37,000 recruits from the year 1982 was also collected but is not used in this study because of the short time span up to the final year of observation of 1990.

The census file contains information on all 6.4 million residents of Finland gathered at the censuses of 1970, -75, -80, -85 and -90. Most importantly, for the purpose of this study, the census file includes information on taxable earnings from the tax administration[4] and detailed information on completed degrees.

Schooling information in the census is based on the Register of Degrees and Examinations compiled by Statistics Finland. The register was created in the 1970 census and supplemented in 1980 with a questionnaire concerning degrees completed before 1970. The register is updated yearly with the information submitted directly by educational institutions. The data contains a five-digit code in which the first digit indicates the level of education. For most of the analysis, degrees completed are converted to years of schooling according to the Standard Classification of Education by Statistics Finland. Individuals who have not completed any post-compulsory education are assigned compulsory nine years of schooling. For a part of the analysis, a discrete grouping is also used classifying levels 1-2 as compulsory, level 3 as vocational, levels 4 and 5 as upper vocational and levels 6 - 8 as university education.

In addition to the records for the recruits, the census data were used to find data on the parents. Information concerning profession, income, education and socioeconomic status of the parents was collected to analyze the effects of the family background. Information on parents was collected from the earliest available census of 1970 so that measures of family background refer to the period when the sample males were about 18 - 20 years of age.

The final data set is constructed by combining information from the census years 1975, -80, -85 and -90. Observations are included from the years when individuals had reached their final (1990) level of schooling and were working full-time[5]. For individuals who appear in more than one census, all the variables are averaged over the years. Due to the inability to identify all the individuals of the original sample from the census data and to missing information on

[4] Statistics Finland customarily top codes the income information in census data so that the actual incomes of the highest 5% are replaced with the average income of that group. For this study uncensored information was available

[5] Data on the months worked is rather unreliable in census. Information is based on a questionnaire. Respondents who did not answer the question on months worked in census were coded to have worked for 0 months. Also, some respondents seem to have (incorrectly) subtracted vacation period from the number of months worked (CSO 1991). Here only those with annual earnings of FIM 50,000 in 1990 currency (approximately 80% of the lowest government salary) or more are considered to be full-time workers.

those who had migrated or died, only 1,537 men remain in the final data. Restricting the analysis to those who had valid information on education and who were full-time workers in at least one census year further reduced the sample size to 1427. Of these, family background information was missing for 421 men so that only 1,016 observations could be used in the analyses involving the effect of family background. Some descriptive statistics of the full-time workers sample that was used in the final estimations are presented in Table 1.

Table 1 Descriptive statistics

	all observations		observations with non-missing family background variables	
	mean	standard deviation	mean	standard deviation
Years of education[a]	11.0	2.1	11.1	2.2
Ed level 2 (compulsory, 9 years)	0.35	0.48	0.33	0.47
Ed level 3 (appr. 10-11 years)	0.37	0.48	0.36	0.48
Ed level 4 (appr. 12 years)	0.16	0.36	0.17	0.37
Ed level 5 (appr. 13-14 years)	0.05	0.23	0.06	0.23
Ed level 6 (appr. 15 years)	0.02	0.15	0.02	0.15
Ed level 7 (appr. 16 years)	0.05	0.21	0.06	0.22
Ed level 8 (more than 16 years)	0.01	0.08	0.01	0.08
Earnings, FIM[b]	112 542	43 680	113 460	45 256
Potential work experience[c]	15.33	2.91	15.28	2.93
Age	33.36	2.84	33.41	2.86
Verbal test score	23.67	8.31	23.88	8.37
Analytical test score	21.83	6.20	21.91	6.16
Math test score	22.88	10.43	23.11	10.36
Lived in Helsinki area	0.11	0.27	0.10	0.27
Lived in other urban area[d]	0.46	0.44	0.44	0.44
Works in the private sector	0.63	0.41	0.60	0.42
Married	0.71	0.46	0.73	0.44
Father’s taxable income in 1970			13 907	13 315
Father upper white-collar			0.04	0.20
Father lower white-collar			0.11	0.32
Father’s education: vocational			0.07	0.25
Father’s educ.: upper vocational			0.07	0.26
Father’s educ.: university degree			0.03	0.17
Father’s information missing	0.29	0.45	0.00	0.00
Observed in 1975	0.69	0.46	0.68	0.47
Observed in 1980	0.81	0.39	0.81	0.39
Observed in 1985	0.86	0.35	0.86	0.35
Observed in 1990	0.86	0.35	0.86	0.34
N	1427		1016	

All figures refer to averages over the years that an individual was a full-time worker in census.

[a] The years of education variable was constructed from information on the highest degree achieved according to the standard educational classification of Statistics Finland.

[b] Annual earned income from tax records. Includes wage and entrepreneurial income but excludes capital income. Converted with CPI to 1990 currency and averaged over years when an individual is observed in census.

[c] Age-years of schooling-7. Average over census years when an individual is observed.

[d] A city where over 90% of inhabitants live in densely populated area.

2.3 OLS estimation results: the effect of ability bias

The earnings differences between groups with different educational levels reflect not only the earnings effects of education but also the effects of the other characteristics of these groups. Notably, it is likely that those with more and less education differ on the average level of ability. Inferences on the effect of education based on the observed earnings differences may well be biased because part of the variation in earnings is caused by the variation in ability.

To give an impression of the ability differences in the sample between individuals having completed different levels of schooling, mean scores on the ability tests by the level of education are reported in Table 2. It appears that mean scores on all the ability tests vary systematically with the level of education. The differences are rather large: for example, the average math test score of university graduates is almost double the average score of those who have completed only the compulsory nine years of schooling.

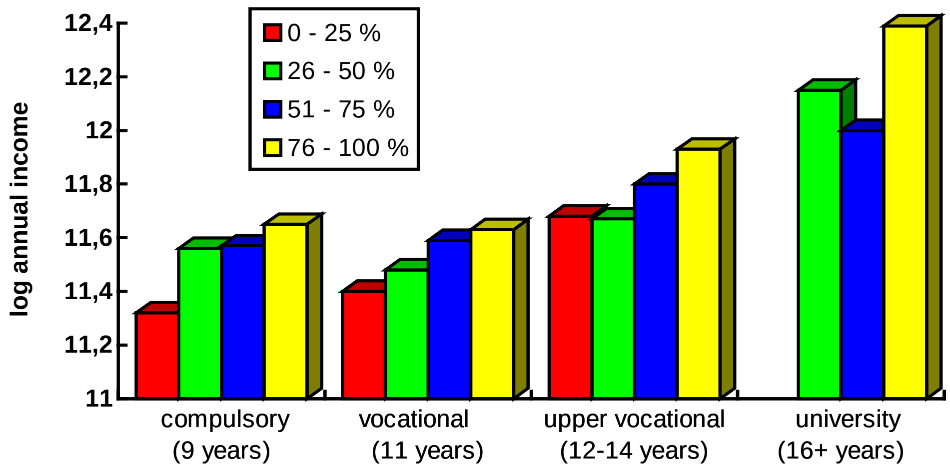
Table 2 Mean ability test scores according to the level of education

	N	math test	verbal test	analytical test
Compulsory education	495	17.6	20.0	19.2
(level 2)		(0.43)	(0.32)	(0.25)
Vocational education	524	21.0	21.6	20.8
(level 3)		(0.41)	(0.32)	(0.25)
Upper vocational educ.	301	31.0	30.0	25.9
(levels 4 – 5)		(0.40)	(0.37)	(0.28)
University education	107	33.4	33.0	27.6
(levels 6 – 8)		(0.48)	(0.52)	(0.42)

Standard errors of means in parentheses

Figure 1 illustrates the effect of ability on earnings with a simple plot. In figure 1, the sample has been divided into four equal sized subgroups according to the percentile rank of the total score in the ability test. Log average annual earnings in 1990 are calculated for these groups at each schooling level and plotted against schooling. As can be seen in Figure 1, groups with higher ability have higher average earnings in all schooling levels. The effect of ability is rather similar in all levels of schooling. Also, average earnings increase more rapidly with the length of schooling in the whole sample than within groups of approximately similar ability, which indicates that the effect of schooling on earnings may be overstated if the ability differences are not accounted for.

Figure 1 Log average annual earnings in 1990 according to the level of education within groups of similar ability



In this study the abilty test scores are utilized to control for the effects of ability. In the basic specification log annual earnings[6] of full-time workers are regressed on a set of schooling variables and ability test scores. In all equations the dependent variable and the time-variant independent variables are averages over the years that the individual was included in the sample. The equations also include controls for (potential) work experience and dummies for region and sector, as well as a set of dummies indicating if an individual was missing from any census year.

The ordinary least squares estimation results presented in Table 3, column (1) indicate that the returns to education are approximately 9.3%[7] when ability differences are not controlled for. This estimate is well in line with earlier studies using Finnish data (Asplund, 1993). The other estimated coefficients also seem reasonable. The experience profile is concave with a one-year difference in work experience increasing earnings by 5% for the first year. Compared to rural areas, earnings are 11.2% higher in the capital area and 5.3% higher in other urban areas. Private sector earnings are approximately 3.3% higher than earnings in the public sector.

[6] Annual earnings are preferred to monthly earnings because the measurement and coding errors in months worked would cause an error in monthly earnings

[7] The percentage differences reported in text are calculated from the antilog of parameter estimates $(e^b-1)*100$, where b is the estimated schooling coefficient in the log earnings equation. For small b the estimated parameters of the log earnings equation are approximately equal to the proportional difference.

Table 3 OLS regression results. Dependent variable is log annual earnings.

	No test scores		Test scores included	
	(1)	(2)	(3)	(4)
Intercept	10.18	11.04	10.26	10.92
Years of education	0.089		0.074	
	(0.006)		(0.006)	
Ed level 3[a]		0.018		0.004
		(0.017)		(0.017)
Ed level 4[a]		0.239		0.189
		(0.025)		(0.027)
Ed level 5[a]		0.352		0.287
		(0.039)		(0.040)
Ed level 6[a]		0.458		0.396
		(0.043)		(0.045)
Ed level 7[a]		0.767		0.701
		(0.053)		(0.054)
Ed level 8[a]		0.737		0.668
		(0.100)		(0.101)
Experience	0.049	0.057	0.046	0.060
	(0.024)	(0.024)	(0.023)	(0.023)
Experience squared	-0.001	-0.002	-0.001	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)
Math test			0.003	0.003
			(0.001)	(0.001)
Verbal test			0.002	-0.000
			(0.001)	(0.001)
Analytical test			0.002	0.003
			(0.002)	(0.002)
Helsinki area	0.106	0.087	0.089	0.075
	(0.025)	(0.024)	(0.025)	(0.024)
Other urban area	0.052	0.052	0.040	0.044
	(0.016)	(0.015)	(0.016)	(0.015)
Private sector	0.032	0.038	0.030	0.038
	(0.016)	(0.016)	(0.016)	(0.016)
N	1427	1427	1427	1427
R squared	0.38	0.43	0.39	0.44

Heteroskedasticity corrected (White 1980) standard errors in parentheses.

All the equations include a set of dummy varaibles indicating if an individual was missing from any of the census years.

[a] Comparison with the reference group “only compulsory education”. For definitions, see Table 1.

In column (3) the three ability test scores measuring mathematical, verbal, and analytical abilities are added to the estimated equation. The ability test scores have an independent positive effect on earnings; mathematical ability, in particular, appears to be important[8].

---

[8] Taubman (1973) found that of the ability measures included in the NBER-Thorndike sample only mathematical ability had a significant effect on earnings. The results in Bishop (1994), based on data from the Armed Forces Vocational Aptitude Battery (ASVAB), indicate that the most important abilities determining earnings of young men were mechanical comprehension and computational speed. Mathematical reasoning ability (covering the high school math curriculum) and verbal ability

Quantifying the effect of ability is not straightforward because the scale of the ability test scores is arbitrary. However, it can be inferred that a man who scores one standard deviation higher on all three tests earns, on average, 6% more than a man with similar education and experience but lower test scores. When the ability measures are included in the regression, all schooling coefficients decrease, indicating that ignoring ability differences leads to a slight overestimate of the average return to education. The coefficient on the years of schooling falls from 0.089 to 0.074. The decrease is statistically significant[9] but the size of the bias does not appear to be very large. Even after accounting for the ability differences, the return to education is reasonably high.

A richer specification, where the effect of education is not restricted to be linear but is allowed to vary according to the level of education yields a similar pattern. First in column (2), where the equation is estimated with no ability measures, the earnings premia associated with educational levels range from low and insignificant 1.8% for vocational schooling (ed level 3) to high of 115% associated with a Master's degree (ed level 7). With the exception of postgraduate degrees (ed level 8) the coefficients of educational dummies increase monotonically with the level of education. All estimated coefficients decrease considerably when the ability variables are introduced in column (4). The coefficient of vocational schooling is practically zero in the regression with ability test scores included. The coefficient of university education decreases by less than 10%, so that after accounting for the ability differences, the earnings premium of university graduates over those with only compulsory schooling is still approximately 100%.

did not have positive effects on earnings. Note, however, that the mathematics section of the Finnish Defense Forces Basic Ability Test used here does not cover high school mathematics but consists of simpler tasks learned by 9th grade.

[9] Under the null hypothesis that ability has no effect, both the estimated schooling coefficients are consistent, but the estimate that excludes ability is efficient. Then the variance of the difference of the two schooling coefficients $\beta_1 - \beta_2$ is the difference of their variances (Hausman 1978). In Table 5.2 $\beta_1 - \beta_2 = 0.014$ with standard error $se(\beta_1-\beta_2) = 0.0024$ yielding a highly significant t-statistic for the hypothesis of equality of the coefficients: $t = 5.9$.

It can be argued that the ability measured by the tests taken while in the army are affected by the schooling completed before the test and, therefore, the effect of ability can not be distinguished from the effect of schooling. After all, at least the tests for mathematical and verbal ability measure skills that are taught in school. However, the inclusion of ability measures in the regression has an effect also on the estimated return to university education which occurs mainly after the test. In any case, the army ability test scores are less dependent on prior schooling than other more school-related measures of ability such as school report cards or final examination results, which are more or less measures of the quality of schooling. Compared with the alternatives, the army tests are more independent and arguably closer measures of the abilities rewarded in the labor market.[10] In addition, only the results of the matriculation examination would be comparable across schools. However, in late 1960's, when the men in this study finished their secondary schooling, only approximately 25% of the age group stayed at school until the matriculation examination, i.e. finished twelve years of general education (Kivinen and Rinne 1995). Thus, the examination results would only cover the upper tail of the schooling distribution.

2.4 Effects of endogeneity of education

The schooling decision is at least in part a result of optimizing behavior of individuals or their parents. This behavior is based on expected outcomes of different choices, i.e. some anticipated earnings functions. To the extent that unobservable (to the econometrician) 'errors' of ex-post and ex-ante earnings functions are correlated, they will induce a correlation between schooling and these unobservable disturbances (Griliches 1977). Controlling for measured ability differences is not sufficient for unbiased estimation, because this correlation may be caused by other unobserved variables.

In this section I present a set of estimation results of earnings equations, where schooling is treated as an endogenous explanatory variable. Family background variables are used as

[10] This argument is supported by Bishop (1994) who found that high-level academic competencies in science and mathematics had no positive effect on earnings of young men. Also Blackburn and Neumark (1995) found that "academic test scores" did not have a significant effect on earnings while "nonacademic tests", particularly, "numerical operations" and "auto and shop information" components of the Armed Services Vocational Aptitude Battery had a significant positive effect on earnings.

instruments that can be excluded from the earnings equation. It is assumed that family background has no direct effect on earnings, but only affects earnings through its effect on schooling. If education is endogenous with respect to earnings, the instrumental variable estimates are consistent, while the ordinary least squares estimates are not. Estimations are performed using two-stage least squares, assuming that years of schooling is a continuous variable. For comparison, a selectivity model with an ordered probit selection rule that captures the discrete nature of the schooling choice is also estimated.

A simple model with endogenous education consists of a two-equation system:

$$\log y_i = \beta S_i + \gamma_1 X_i + \varepsilon_{1i}$$

$$S_i = \gamma_2 Z_i + \varepsilon_{2i}. \qquad (1)$$

Earnings (y_i) of individual i are determined by schooling (S_i) and a vector of exogenous variables (X_i) including, most importantly, work experience and ability. Z_i is a vector of exogenous individual characteristics that influence the schooling decision. The most influential variables in Z are the ability and family background variables. The vectors X and Z are overlapping, with ability variables appearing in both equations. Family background variables are excluded from X to identify the earnings equation.

Education is not really a continuous variable but rather an ordered set of different levels. The discrete nature of education is captured in an ordered probit[11] model that is used here as an alternative estimation method. Earnings equations can then be estimated using a selectivity correction. In an ordered probit model, the optimal amount of schooling is not observed. What is observed is the discrete level of education closest to the desired amount. Thus, the actual level of schooling chosen depends on the optimal amount falling between certain threshold values. These thresholds can be estimated with an ordered probit together with the coefficients of the exogenous variables.

[11] Another widely used method in the case of several discrete choices is a multinomial logit model. In the multinomial logit model the effects of the exogenous variables on the choice probabilities are estimated. The choices are assumed to be independent and individuals choose the one giving the highest utility. However, the multinomial logit fails to account for the ordinal nature of the dependent variable and is therefore less effective than the ordered probit.

In the discrete case the model for schooling and earnings is:

$$\log y_i = \beta S_i + \gamma_1 X_i + \varepsilon_{1i}$$

$$S^*_i = \gamma_2 Z_i + \varepsilon_{2i}$$

$$S_i = j \ \text{ iff } \mu_{j-1} < S^*_i \leq \mu_j, \qquad j = 0, 1, 2, 3 \tag{2}$$

where y is earnings and S the observed level of schooling that depends on the underlying latent optimal length of schooling choice variable S*. The threshold parameters μ_j are unknown and are estimated simultaneously with γ_2. The schooling choice probit model is estimated with maximum likelihood, assuming that the error term in the schooling equation is normally distributed with zero mean and unit variance (and fixing the intercept by setting $\mu_0 = 0$). The selectivity correction involves calculating the expected value of earnings conditional on the chosen level of schooling.

$$\begin{aligned} E(y_i|S_i=j) &= \gamma_1 X_i + \beta S_i + E(\varepsilon_{1i}|\ S_i=j) \\ &= \gamma_1 X_i + \beta S_i + E(\varepsilon_{1i}|\ \mu_{j-1} - \gamma_2' Z < \varepsilon_{2i} \leq \mu_j - \gamma_2' Z). \end{aligned} \tag{3}$$

Since the two error terms are correlated, the conditional expectation of the earnings equation error, $E(\varepsilon_{1i}|S_i=j)$, is generally not zero. Instead, it depends on the conditional expectation of the error term in the schooling equation (ε_{2i}), given the observed level of schooling. The non-zero expectation results from the endogenous choice of education. Assuming that the error terms have a bivariate normal distribution with zero means (in the population) and correlation ρ, the expectations can be calculated from the moments of the truncated normal distribution (Maddala 1983: 366).

$$E(\varepsilon_{1i}|\ \mu_{j-1} - \gamma_2' Z < \varepsilon_{2i} \leq \mu_j - \gamma_2' Z) = \rho\sigma_{\varepsilon 1} E(\varepsilon_{2i}|\ \mu_{j-1} - \gamma_2' Z < \varepsilon_{2i} \leq \mu_j - \gamma_2' Z)$$

$$= \rho\sigma_{\varepsilon 1} \frac{\phi(\mu_{j-1} - \gamma_2' Z) - \phi(\mu_j - \gamma_2' Z)}{\Phi(\mu_j - \gamma_2' Z) - \Phi(\mu_{j-1} - \gamma_2' Z)} = \rho\sigma_{\varepsilon 1}\lambda, \tag{4}$$

where $\sigma_{\varepsilon 1}$ is the standard error of the disturbance term in the earnings equation and $\phi(.)$ and $\Phi(.)$ are, respectively, the density function and the distribution function of the standard normal distribution.

Estimation results

The results of the first stage regression of schooling on ability and family background variables are presented in Table 4. The reduced form least squares and ordered probit coefficients are not directly comparable, since in the least squares equation, the dependent variable is years of schooling, while in the ordered probit it is a discrete level of schooling. However, the results are qualitatively similar with the father's education and ability variables having a highly significant impact on the length of schooling. The family background variables that are to be excluded from the earnings equation are jointly significant in the schooling equation and can, therefore, be used as instruments for schooling.[12] The effect of ability on schooling choice can be calculated from the parameter estimates of the reduced-form OLS equation in the same way as the effect of ability on earnings in section 2.3. One standard deviation increase in all the test scores increases schooling by 0.6 years. The impact calculated using coefficients from a regression of schooling on family background and ability variables only, without controlling for the other covariates of the earnings equation, is 1.2 years. The high predictive power of reduced form least squares is caused partly by inclusion of earnings equation covariates, especially work experience.

[12] Father's income was originally also used as an instrument but since it was insignificant in the schooling equation and caused problems with the specification tests in the earnings equation, it was switched to the set of explanatory variables in the earnings equation. Father's income apparently has also a direct effect on earnings.

Table 4 First stage regressions for schooling

	Reduced form OLS[a]		Ordered probit	
	coefficient	standard error	coefficient	standard error
Intercept	17.612	0.780	-1.721	0.130
Math test	0.019	0.005	0.039	0.006
Verbal test	0.038	0.007	0.036	0.006
Analytical test	0.013	0.009	0.021	0.009
Father upper white-collar	0.323	0.231	0.395	0.177
Father lower white-collar	0.133	0.130	0.234	0.127
Father university educ.	0.991	0.259	0.971	0.231
Father upper voc. educ.	0.507	0.161	0.415	0.156
Father vocational educ.	0.078	0.143	0.217	0.129
μ_1			1.255	0.060
μ_2			2.098	0.077
μ_3			2.522	0.086
μ_4			2.749	0.092
N	1016		1016	
F-test for excluded instruments[b]	F(5,997)=9.53 p=0.00			
R squared	0.73			
Log likelihood			-1237	

[a] All the equations include a set of dummy varaibles indicating if an individual was missing from any of the census years. Reduced form OLS equation also includes all the covariates of the earnings equation.
[b] The instruments that are to be excluded from the earnings equation are: indicator variables of father's socio-economic status (upper white-collar, lower white-collar) and father's education (university, upper vocational, vocational).

The estimation results from the different earnings equation specifications are reported in Table 5. In the first column, the equation of Table 3, column (3) is re-estimated with ordinary least squares using only the observations with nonmissing family background variables to ensure that the difference between the OLS and IV estimates is not caused by sample selection. The results in this subsample are not very different from the full sample estimates.

Table 5 Wage equations with endogenous education. Dependent variable is log annual earnings.

	OLS	IV (2SLS)[a]	IV (2SLS)[a]	selectivity corrected[b]
	(1)	(2)	(3)	(4)
Intercept	10.08	8.69	9.10	9.47
Years of education	0.081	0.157	0.129	0.124
	(0.007)	(0.017)	(0.018)	(0.013)
Math test	0.003	0.001	0.002	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Verbal test	0.001	-0.003	-0.001	-0.002
	(0.001)	(0.002)	(0.002)	(0.002)
Analytical test	0.002	0.001	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Experience	0.057	0.109	0.089	0.081
	(0.025)	(0.034)	(0.034)	(0.028)
Experience squared	-0.001	-0.002	-0.002	-0.002
	(0.001)	(0.001)	(0.001)	(0.001
Log father's income			0.014	0.014
			(0.005)	(0.005)
λ[c]				-0.097
				(0.024)
N	1016	1016	1016	1016
R squared	0.40	0.33	0.38	0.42
Hausman test[d]		t=2.50	t=1.53	
		p=0.01	p=0.12	
Overidentification test		$\chi^2(5)=13.05$	$\chi^2(4)=5.60$	
		p=0.02	p=0.23	

The estimated equations also include same additional dummy variables for region and sector as table 3 as well as indicators for missing data on any census. Standard errors are in parentheses.

[a] The set of instruments that are excluded from the earnings equation includes dummy variables for father's education, father's socioeconomic status and the place of residence in 1970 (See table 4). In column (2) the set of instruments also includes father's income while in column (3) father's income is among the regressors.

[b] Calculating standard errors in the ordered probit is rather complicated. The residuals of the ordered probit equation come from several truncated distributions. The correction used here is programmed in the LIMDEP manual, p. 628 (Greene 1991).

[c] Inverse Mills' ratio, $E(\varepsilon_2 |S=j)$.

[d] Test for the hypothesis that the OLS and IV coefficients are equal, performed by testing with the t-test the significance of the coefficients of the fitted values from the first-stage instrumental variables regression in the log wage equation estimated with ordinary least squares. (Davidson and MacKinnon 1993: 239)

The results from the instrumental variable estimation are presented in columns (2) and (3). In column (2), the set of excluded instruments contains all the family background variables. The schooling coefficient rises to almost 0.16 and schooling appears to be endogenous according to the Hausman test. However, overidentification restrictions requiring that all instruments are orthogonal to the earnings equation error are rejected.[13] A prime candidate for a nonvalid instrument is the father's income which appears to have a direct effect on the son's earnings. When the father's income is included in the earnings equation in column (3), the overidentification restrictions are not rejected. The return to schooling estimate is now 0.129, which is still clearly higher than the OLS-estimate of 0.081 but the Hausman test no longer rejects the null hypothesis of equality of OLS and IV coefficients. The difference between IV and OLS estimates is similar in magnitude to the estimates of Card (1993) but somewhat smaller than in Harmon and Walker (1995). It is also interesting to note that the coefficients of the ability variables decrease and lose their significance in the IV estimation.

Estimation of the selectivity-corrected earnings equation with the ordered probit selection function in column (4) produces an estimate for the return to education that is also higher than the OLS estimate. Endogeneity is supported by the significance of the selectivity correction term λ. The estimate for λ is negative, which implies that the least squares estimates are biased downwards.

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[13] The essential idea of the test is that, after controlling for the other covariates, the excluded instruments should have no explanatory power in the earnings equation. The easiest way to perform this overidentification test is to regress the residuals from the two-stage least squares estimation on all the included explanatory variables and the excluded instruments. It can be shown that under the null hypothesis of no correlation between the instruments and the error term of the earnings equation, nR^2 from this regression is asymptotically χ^2(l-k)-distributed, where l-k is the number of overidentification restrictions (Davidson and MacKinnon 1993).

So far the topic of this section has been the endogeneity of education. However, if the number of years spent in school is endogenous, work experience, defined as the number of years at work after school, must also be endogenous.[14] Table 6 presents estimation results that are consistent when experience is endogenous. In the first two columns, work experience is replaced with age which can safely be treated as an exogenous variable. The interpretation of the coefficient on education is now the net effect of spending an additional year in school rather than gaining work experience. Here the schooling coefficient from the instrumental variable regression exceeds the OLS estimate by almost 60%. In column (3) the equation is estimated with two-stage least squares treating experience as endogenous and using age and age squared as additional instruments. The resulting schooling coefficient is 0.11, only slightly lower than in Table 5 where experience was treated as exogenous. Interestingly, the coefficient of experience also decreases and is no longer significant at conventional levels.

[14] A more convincing test for the endogeneity of experience would require information on actual years of work experience. Since labor supply is expected to depend on wages, accumulated labor supply will depend on an individual's history of wages. Fixed components in the wage equation error could lead to current experience being correlated with the current error. As schooling and experience are correlated, inconsistency in the experience coefficient estimate can carry over to the schooling coefficient estimate (Blackburn and Neumark 1995). However, treating work experience as endogenous may not be irrelevant even when information is available only on potential work experience. The measure of work experience depends on the length of schooling. If the length of schooling is endogenous, the work experience measure may also be correlated with the error term.

Table 6 Wage equations with endogenous education and experience. Dependent variable is log annual earnings.

	OLS age proxying experience (1)	IV (2SLS) age proxying experience (3)	IV(2SLS) endogenous experience[a] (3)
Intercept	10.17	9.00	9.95
Years of education	0.066	0.104	0.110
	(0.005)	(0.022)	(0.021)
Math test	0.003	0.001	0.001
	(0.001)	(0.002)	(0.002)
Verbal test	0.001	-0.002	-0.001
	(0.002)	(0.002)	(0.002)
Analytical test	0.002	0.001	0.001
	(0.002)	(0.002)	(0.002)
Log father's income	0.016	0.014	0.014
	(0.004)	(0.005)	(0.005)
Age	0.013	0.072	
	(0.083)	(0.092)	
Age squared	-0.000	-0.001	
	(0.001)	(0.001)	
Experience			0.033
			(0.073)
Experience squared			-0.001
			(0.002)
N	1016	1016	1016
R squared	0.41	0.37	0.38
Hausman test		t=1.79	F(3,998)=1.42
		p=0.07	p=0.23[b]
Overidentification test		$\chi^2(4)=3.91$	$\chi^2(4)=4.33$
		p=0.42	p=0.36

The estimated equations also include same additional dummy variables for region and sector as table 3, as well as indicators for missing data on any census. Standard errors are in parentheses.

The set of instruments (for both schooling and experience) includes dummy variables for father's education and father's socioeconomic status.

[a] In column 3 age and age squared are used as additional instruments.

[b] Joint test of significance for the fitted values of schooling and experience from the first-stage regression in the log earnings equation.

While the returns to years of education give an impression of the average effects of education, it may be more meaningful to study the returns to educational credentials. The return to a year in school may vary according to the level of schooling. A year at a university is not equivalent to a year in a vocational school. And, since the highest level of completed education is the information that is actually recorded in the data, it is probably more reliable than an artificially constructed measure of years of education.

Table 7 shows the results obtained when schooling is measured by the highest degree completed. In the first column are the OLS estimates for the subsample with non-missing family background information. In column (2) selectivity correction is applied using an ordered probit selection function. Columns (3) and (4) repeat the analysis using wider educational categories classifying levels 4 and 5 as upper vocational education and levels 6, 7 and 8 as university education. This grouping is used in estimating both selection and earnings functions.

The reference category in the equations of Table 7 is individuals with only a compulsory education. According to the OLS estimation, there are significant returns to all levels of education except vocational schooling the effect of which is practically zero. These results are similar to the full sample estimates.

The selectivity-corrected estimation results in column (2) again indicate an increase in the estimated effect of education when the endogeneity of education is taken into account. According to these estimates, a man with vocational schooling earns about 7 % more and a man with a university degree about 140% more than he would have earned had he started working directly after compulsory school.

The same pattern is visible also in columns (3) and (4) where education levels are not as narrowly defined. Selectivity correction leads to a systematic although not significant increase in estimated returns to educational credentials. The selectivity correction term is negative but insignificant in both columns (2) and (4).

Table 7 Returns to qualifications. Dependent variable is log annual earnings

	OLS (1)	Selectivity corrected (2)	OLS (3)	Selectivity corrected (4)
Intercept	10.64	10.67	10.82	10.85
Ed level 3[a]	0.013	0.067	-0.005	0.047
	(0.022)	(0.050)	(0.022)	(0.051)
Ed level 4 [a]	0.176	0.265		
	(0.030)	(0.079)	0.174	0.265
Ed level 5 [a]	0.281	0.392	(0.030)	(0.084)
	(0.043)	(0.101)		
Ed level 6 [a]	0.425	0.543		
	(0.061)	(0.115)	0.569	0.702
Ed level 7-8 [a]	0.727	0.873	(0.052)	(0.127)
	(0.059)	(0.135)		
Math test	0.003	0.002	0.003	0.002
	(0.001)	(0.002)	(0.001)	(0.002)
Verbal test	-0.001	-0.002	-0.000	-0.002
	(0.001)	(0.002)	(0.001)	(0.002)
Analytical test	0.003	0.002	0.003	0.003
	(0.002)	(0.002)	(0.002)	(0.001)
Experience	0.078	0.080	0.062	0.063
	(0.025)	(0.025)	(0.025)	(0.025)
Experience squared	-0.002	-0.002	-0.002	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)
λ[b]		-0.042		-0.041
		(0.035)		(0.035)
N	1016	1016	1016	1016
R squared	0.45	0.46	0.44	0.44

Equations also include dummy variables for region and sector as well as a set of dummy varaibles indicating if an individual was missing from any of the census years. Standard errors are in parentheses.
[a] Comparison to the reference group "only compulsory education".
[b] Inverse Mills' ratio, $E(\varepsilon_2 |S=j)$.

According to all the estimation results reported above, two-stage methods that take into account the endogeneity of education produce systematically higher estimates for the effect of education on earnings. This result implies that there is a negative correlation between schooling and the earnings equation residual. This is a rather nonintuitive result; common sense would suggest that unobserved 'good' characteristics should have a positive effect on both earnings and schooling. Hence, the correlation between the residuals in the earnings equation and the schooling equation should be positive. However, the estimation results reported above as well as in previous studies based on the IV approach (Card 1993, Harmon and Walker 1995) suggest the opposite.

2.5 Conclusion

Since individuals with different amounts of schooling generally differ also by other observed and unobserved characteristics, ordinary least squares estimates based on comparison across individuals may not reflect the true returns to schooling. In this paper I estimate returns to education controlling for individual abilities by including ability test scores in the earnings equations. I also treat education as endogenously determined or erroneously measured and use family background variables as instruments for education.

The first question addressed in this paper concerns the effect of cognitive abilities, as measured by the army test scores in 1970. These abilities are found to have a significant and fairly large effect both on the choice of the length of schooling and on subsequent earnings. Also, ignoring individual ability differences apparently overstates the profitability of education, although the bias in the estimated return to years of schooling is not very large. Introducing ability measures in the earnings equation decreases the estimated effect of years of schooling on earnings from 9.3% to 7.7%

Taking into account the endogeneity of schooling suggests, however, that ordinary least squares estimates are subject to a downward bias. Even after controlling for ability differences, estimates using instrumental variables or selectivity correction techniques produce estimates of the return to education in the range of 11-13%, significantly higher than ordinary least squares estimates. These estimates are similar to those obtained by Card (1993) and Ashenfelter and Krueger (1994) with U.S. data and Harmon and Walker (1995) with U.K. data. The results suggest that the unobservable disturbances in the equations that determine schooling and earnings are negatively correlated. Those who have completed unexpectedly high amounts of schooling, given their family background and ability test scores, are those who, for whatever reason, have lower than average initial earnings capacity.

There are at least three distinct explanations for the difference between ordinary least squares and instrumental variables estimates. First, the least squares estimates may be downward biased because of measurement error in schooling. Instrumental variable estimates are consistent also in the presence of measurement errors. Second, the excluded instruments may also have a direct effect on earnings. While overidentification tests generally did not reject the hypothesis that family background can legitimately be excluded from the earnings equation, the possibility of misspecification, that would bias the instrumental variable estimates upwards, remains. Third, optimizing behavior by individuals may result in a correlation between optimal schooling and the earnings equation error which leads to a bias in the OLS estimates.

There is no clear-cut test to discriminate between these explanations. However, it appears unlikely that measurement error could fully explain the difference between OLS and IV estimates. In survey data, the reliability of schooling measures is typically estimated to be around 90% (Ashenfelter and Krueger, 1994), which would attenuate the schooling coefficient by 10% in a bivariate regression with schooling as the only explanatory variable. In the administrative data used here, measurement error is likely to be smaller. When the estimated equation includes several correlated variables that may all be erroneously measured, the effect of measurement errors on estimates is much more complicated, but the measurement error in schooling would have to be very large to induce a bias of the magnitude found here.

However, it is tempting to note that the difference between IV and OLS estimates is in accordance with models of the optimal choice of schooling. Schooling is cheaper for individuals who have less income to forego while in school and, therefore, the optimal amount of schooling is larger. In terms of Griliches (1977), optimized schooling and the unobserved disturbance in the earnings equation may be negatively correlated if there is also “another unmeasured individual income generating factor unrelated to ability (for example, motivation or energy)” which only increases potential earnings and therefore marginal costs of schooling with no effect on the marginal return to schooling. A negative correlation between schooling and the error term is likely to bias OLS estimates downward.

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Chapter 3

Estimating heterogeneous treatment effects in the Becker schooling model[1]

Abstract

In this paper, we provide evidence that the rate of return to schooling varies across individuals and this variation is responsible for much of the variation in the length of schooling. The varying returns imply a random coefficient model of earnings determination. We show that instrumental variable estimators still consistently estimate the average return to schooling under reasonable assumptions and that the IV-estimates are almost identical to more general control function estimates. Our maximum likelihood estimates imply that variation in the returns to schooling is related to ability differences while family background only affects the discount rates.

Keywords: Returns to schooling, random coefficient models, IV-estimators

3.1 Introduction

The empirical literature on program evaluation, including the impressive body of literature on returns to schooling, typically frames selection bias as an omitted variable problem. If treatment, the degree of participation in a social program, is positively or negatively correlated with unobservables that affect the outcome of interest, then ordinary least squares estimates of the average effect of treatment will absorb the effects of these unobservables, and will be biased accordingly. This selection problem will occur when program participation is endogenously determined by the same unobservable factors which influence the outcome. Given this omitted variable problem, instrumental variable estimators or appropriate two-step selection correction methods consistently estimate treatment effects.

[1] Joint work with Karen Conneely.

However, the traditional concept of selection bias as a problem of omitted variables is overly narrow. According to economic theory, individual participation decisions should weight equally the costs and benefits of participation. The simple selection problem described above reflects only cost-based selection. As we discuss below, the endogeneity problem due to selection based on variable benefits is different and more severe.

Variation in benefits implies positive selection, since individuals with higher benefits to participation in a program are more likely to participate, ceteris paribus. In this case, the endogenous treatment variable is influenced by its own coefficient, yielding an endogeneity problem that is not accounted for in most selection-correction models. This problem will exist unless the benefits of the program are truly constant across individuals. However, the assumption of constant benefits is probably unrealistic. It seems likely that individuals who are heterogeneous in all other dimensions, including the observed regressors, unobserved earnings potential, and costs of participation, will also vary in their potential to benefit from a social program.

In the empirical literature, the benefits to a program are commonly treated as a single population parameter to be estimated, even when it is accepted that benefits vary. Often it is more feasible to estimate a single parameter of interest, such as the average benefit to a year of education, than an entire distribution of benefits. However, estimates of the average benefit of the treatment may be biased upward if actual benefits vary.

Still, much of the theoretical literature acknowledges the presence of heterogeneity in returns without addressing the selection bias implied by this heterogeneity. Angrist and Imbens (1995) show that the IV-method can be used to calculate average causal responses to treatment even in the case when the responses vary between individuals. Implicit in their analysis, however, is the assumption that the expected responses do not affect the choice of the level of treatment. In a related paper, Imbens and Angrist (1994) show that the IV-estimates identify local average treatment effects, that is, the average effect of treatment on the people who were induced to change their participation status by a change in the instrument. Again, individual variation in treatment effects is acknowledged but the possibility that this variation may influence participation is not.

Heckman (1995,1997) shows that instrumental variables estimates of treatment effects are consistent only if (a) the effect of treatment is the same for everyone (conditional on observables) or (b) treatment effects are variable across people but do not affect the decision to participate in the program. While condition (a) is typically assumed in the empirical literature and condition (b) is implicitly assumed in the work by Angrist and Imbens (1995), Heckman argues that neither of these assumptions is realistic. Condition (b), the less restrictive of the two conditions, requires that the persons being studied do not have better information than the econometrician on the likely effects of the program. Since this assumption requires extremely rich data available to the econometrician, Heckman concludes that the method of instrumental variables is likely to fail in estimating the effects of treatment on the treated.

Heckman and Robb (1985) and Garen (1984) discuss the use of control function estimators, which include terms that proxy the variable component of the coefficient. These estimators generally require assumptions that are somewhat stronger than those conventionally used to guarantee consistency of the IV-estimators, but are weaker than the more general IV-assumptions discussed above. In the case of dichotomous treatment these estimators are usually dependent on distributional assumptions and hence very different from their IV counterparts. In the case of continuous treatment with linearity assumptions (as in Garen 1984), the appropriate control function estimator is simply a more general IV-estimator requiring only slightly weaker assumptions.

Under this framework, we consider the consistency of instrumental variables estimation of average treatment effects when the effects may vary across individuals, and analyze the resulting estimation problems in the familiar context of estimating the returns to schooling. In section 3.2 we present the continuous-treatment evaluation problem in terms of the Becker schooling model, yielding a variant of the random coefficients model. Section 3.3 descibes the data. The IV and control function estimates are presented in section 3.4. Finally, in Section 3.5 we use maximum likelihood to estimate a joint model of wage determination and schooling. Section 3.6 wraps up with a brief conclusion.

3.2 Variable returns to schooling and related estimation problems

A common example of an estimation problem of the effects of continuous treatment is the classic case of the returns to schooling. While most of the empirical literature on this topic has not dealt with the possibility of individual variation in returns to schooling, the idea has received attention in several more theoretical treatments of the topic. Variation in returns is implicit in the dichotomous schooling choice model of Willis and Rosen (1977), who analyze the decision to attend college. Card (1994), in a re-interpretation of Becker (1967), presents a model of endogenous schooling determination where schooling choice is continuous and is influenced by varying returns. This model is consistent with the stylized facts about the determinants of schooling and the relation between schooling and earnings, predicting that variation in family background and ability will have a strong effect on schooling choice and that the relationship between schooling and log earnings is approximately linear.

We adopt Card's version of the Becker model and assume that individuals choose schooling to maximize a utility function (U) defined over average earnings (y) and years of schooling (S):[1]

$$U(y,S) = \ln y - f(S) \qquad (1)$$

where f(S) is an increasing convex function representing the disutility of attaining schooling level S. Individual opportunities can be summarized by a log-concave function y = g(S), which represents the level of earnings available at each level of education. This yields the first order condition for optimal schooling:

$$\frac{g'(S)}{g(S)} = f'(S)\,. \qquad (2)$$

In the standard case ln y=a+bS and f(S)=rS, where b is the marginal return and r the marginal cost. Now, the optimization problem is equivalent to maximizing discounted lifetime earnings for infinitely lived individuals: $\int_S^\infty g(S)e^{-rt}dt = \frac{g(S)e^{-rS}}{r}$. The first-order condition (2) above reduces to b=r, so optimal schooling equates the marginal rate of return to schooling with the marginal cost of schooling or marginal rate of substitution between current

[1] For a similar derivation see Ashenfelter and Rouse (1996).

and future earnings.[2] If b and r are constant in schooling, the solution to the optimization problem will be a corner solution: $S^*=0$ if $b<r$; $S^*\rightarrow\infty$ if $b>r$. An interior solution can only be guaranteed with the assumption that either marginal returns or marginal costs vary with the level of schooling.

As in Card (1994), we operationalize the model by assuming that marginal costs are increasing linear functions of schooling. To keep our analysis simple, we diverge from Card's model with our assumption that marginal returns do not vary with schooling, although we will relax and test this restriction when we estimate the model with maximum likelihood. To allow for individual heterogeneity in marginal returns and marginal costs, we assume person-specific intercepts but assume a homogenous slope for marginal costs:

$$\frac{g'(S)}{g(S)} = b_i \tag{3}$$

$$f'(S) = r_i + kS \tag{4}$$

Equalizing marginal costs and benefits yields an optimal level of schooling for individual i:

$$S_i^* = \frac{b_i - r_i}{k} \tag{5}$$

In this model, schooling choices vary across individuals for two reasons: because individuals have different rates of return to schooling (variation in b_i), and because individuals have different rates of substitution between schooling and future earnings (variation in r_i). Card (1994) speculates that variation in b_i corresponds to variation in ability while variation in r_i corresponds to variation in access to funds or tastes for schooling. Denoting the population means of b_i and r_i as $\bar{b}$ and $\bar{r}$ we can rewrite returns and costs as :

$$b_i = \bar{b} + \nu_{bi}, \tag{6}$$

[2] We use the terms marginal costs and (marginal) discount rates interchangeably. Both refer to the derivative of function f (S) with respect to schooling. In our model, the discount rates and the marginal costs of schooling are not separately identified.

$$r_i = \bar{r} + \nu_{ri}. \quad (7)$$

Integrating the marginal returns equation yields our implicit assumption of the standard log-linear earnings equation for individual i:

$$\ln y_i = a + b_i S_i + \varepsilon. \quad (8)$$

If marginal returns to schooling vary between individuals then the above equation can be treated as a random coefficients model:

$$\ln y_i = a + (\bar{b} + \nu_{bi})S_i + \varepsilon. \quad (9)$$

Note that because schooling depends on the return, b_i, there is positive correlation between S_i and its random coefficient. This variant of the random coefficients model is not commonly dealt with in the literature. The textbook random coefficients model[3] treats random coefficients as white noise, assuming that there is no correlation between the random individual component of the slope coefficient and any of the other regressors. In the textbook case, the standard OLS or IV methods yield consistent, but inefficient estimates. However, such a model is not a realistic descriptor of schooling choice if individuals have private information about their ability to benefit from additional schooling.

The above random coefficient model can be rewritten as

$$Y = a + \bar{b}S + (\varepsilon + \nu_b S) = a + \bar{b}S + U. \quad (10)$$

The interesting parameter here is the average return to a year of schooling, $\bar{b}$,[4] but the OLS estimates of $\bar{b}$ will be inconsistent if S is correlated with the error term. In this case, the OLS estimates will suffer from two separate types of bias since S may be correlated with both ε and $\nu_b S$.

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[3] See, for example, Greene (1993)

[4] Note that contrary to a discrete choice case, the average population return to education is the interesting parameter when treatment is continuous. In a discrete choice case it might be more interesting to estimate the effect of treatment to some subpopulation actually receiving treatment. In the continuous case everyone receives treatment, but in varying intensities.

S and ε will be positively correlated if individuals with higher potential earnings tend to attain more schooling, perhaps because of lower costs or disutilities of schooling ($E(\varepsilon\nu_r) < 0$). In this case, the OLS estimates will overstate the return to schooling. If this were the only source of bias (i.e. if ν_b=0) then consistent estimates of $\bar{b}$ could be obtained with instrumental variables estimators or simple selection correction. For the IV estimates to be consistent, the instrument Z must be correlated with the endogenous variable (S) but uncorrelated with the error (U):

A1) $E(S|Z) \neq E(S)$;

A2) $E(U|Z) = 0$.

If benefits of the program are constant across individuals (ν_b=0), then U is simply ε, so these conditions are met by variables which affect the costs of schooling but not earnings. Potential instruments could include measures of family background, as well as variables which mimic random assignment.

When benefits or the program vary, these conditions cannot be satisfied. In a population of rational, well-informed individuals, those with a higher return to a year of schooling, should attain more schooling than those with lower returns. Hence, S will be positively correlated with ν_b, violating the condition $E(U|Z) = E(\varepsilon + \nu_b S\ |Z) = 0$. Also, A1 explicitly states that S must be correlated with Z in some way, so $\varepsilon + \nu_b S$ is likely to depend on the instrument Z.

Using this argument, Heckman and Robb (1985) demonstrate the general inconsistency of instrumental variables estimators in random coefficients models. Their proposed solution to the random coefficient estimation problem is control function approach, similar to the two-step procedure commonly used to correct for traditional selectivity bias. In this approach, the participation decision is modeled to obtain consistent estimates of $E[\varepsilon + \nu_b S\ |S, Z]$, which are then plugged into an OLS regression of earnings on schooling:

$$E[Y|S,Z] = a + \bar{b}\ S + E[\varepsilon\ |S,Z] + S \cdot E[\nu_b\ |S,Z]. \qquad (11)$$

This approach requires that the conditional expectation of the unobservables, ν_b and ε, can be specified up to unknown parameters in terms of the observables S and Z. This is usually done by specifying the functional form of the participation decision and the relationships between the unobservable terms. Since schooling is a roughly continuous variable, it seems natural to specify a linear relationship between S and Z:

$$S = \gamma Z + \eta \tag{12}$$

where $\gamma \neq 0$ and η is uncorrelated with Z:

CF1) $E(\eta|Z) = 0$.

Depending on the assumptions one is willing to make about the conditional expectations in (11) several estimation strategies are available. In a linear case the most common assumption is that conditional expectations of the outcome equation are linear in $\eta = S - \gamma Z$.

CF2) $E[\varepsilon\,|S, Z] = E[\varepsilon\,|\eta] = c\,\eta$

$E[\nu_b\,|S, Z] = E[\nu_b\,|\eta] = d\,\eta$.

Given the relationship specified in CF2, the appropriate control function estimate of $\bar{b}$ would be obtained by obtaining the OLS residuals $\hat{\eta}$ from the reduced form schooling equation (12) and estimating the following OLS regression:

$$Y = a + \bar{b}\ S + \lambda_1\ \hat{\eta}\ + \lambda_2\ \hat{\eta}\, S.\tag{13}$$ [5]

Garen (1984) uses this control function approach to estimate the return to schooling in a random coefficients framework, asserting that the IV estimates are inconsistent in this type of model due to violation of the condition E[U|Z]=0. He specifies a model similar to the one discussed here, except for the additional assumption of joint normality of the unobservables.

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[5] The standard errors from this regression will need to be corrected for the presence of estimated regressors and heteroskedasticity of known form. Rewriting equation (13) as $Y = X'\beta + u$, $Var(\beta) = (X'X)^{-1}\,(X'VX + (X'DZ)\,\sigma_\eta^2(Z'Z)^{-1}\,(Z'DX))\,(X'X)^{-1}$, where D is a diagonal matrix containing the derivative of Y with respect to $\hat{\eta}$ (in this model $D_i = \lambda_1 + \lambda_2\,S$). V is a diagonal matrix containing the estimated variance of the disturbance term u. In this model $V_i = \sigma_{\varepsilon|\eta}^{\ 2} + 2S_i\,\sigma_{b\varepsilon|\eta} + S_i^2\sigma_{b|\eta}^{\ 2}$. Provided that the disturbances are homoskedastic with respect to each other, V_i is a function of schooling only and can be estimated using the predicted values from a regression of the squared residual from equation (13) on S_i and S_i^2.

This additional assumption is sufficient but overly strong for consistency of a control function estimator. It not only generates conditions CF1 and CF2, but imposes homoskedasticity of all unobservables. We will show below that this guarantees the consistency of the IV estimator as well.

Note that if the final term were left out of the regression, that is, if λ_2 were restricted to zero, equation (13) would be equivalent to a simple two-step selection correction. When the participation decision is linear, however, the two-step selection correction is numerically equivalent to instrumental variables. Hence, the control function specified above is just a less restrictive form of instrumental variables.

In addition to CF1 and CF2, the only extra condition required for consistency of the IV estimator, using Z as the instrument, is the assumption that η is homoskedastic: $E(\eta^2|Z) = \sigma_\eta^2$. This is obviously not satisfied in a discrete choice case with a linear probability model, but may be quite reasonable in a continuous case. Given CF1, CF2, and homoskedasticity of η, and using the law of iterated expectations

$$E(\varepsilon + \nu_b S\,|Z) = E[E(\varepsilon + \nu_b S\,|Z, \eta)|Z] = E[c\eta + d\eta(\gamma Z+\eta)|Z]$$

$$= (c + d\gamma Z)\, E[\eta|z] + dE[\eta^2|Z] = d\sigma_\eta^2 = E(\varepsilon + \nu_b S). \qquad (14)$$

As noted by Robinson (1989), $E(U|Z) = E(U)$ is sufficient for consistent IV estimation of all parameters in the earnings equation except the intercept term. The assumption deviates from A2 only in that the error term is unconditionally biased; this bias is simply absorbed by the intercept term and all other parameters can be estimated consistently with IV[6].

So given the requirements for the control function estimator and the additional assumption of homoskedasticity, IV estimator will consistently estimate all of the interesting parameters even when schooling is correlated with its coefficient. If these assumptions can be reasonably expected of a typical instrument, then this is good news for the multitude of instrumental

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[6] Wooldridge (1997) shows that the IV estimates are consistent in the random coefficient model using essentially similar arguments. He also presents an alternative set of assumptions that could be applied to treatment if the participation choice follows a linear probability model.

variables estimates of the return to education. These assumptions are stronger than the usual requirements for IV, and credible instruments are hard enough to find without the extra restrictions. However, many of these assumptions are implied by other common (but stronger) assumptions. For example, true independence of η and Z (as opposed to mean independence) will guarantee homoskedasticity of η conditional on Z. Although stronger than necessary, independence is often explicitly assumed in the theoretical literature since it yields consistency results for a broader class of models. It is also implicit in most distributional assumptions, including joint normality of unobservables, which also generates CF1 and CF2.

The control function estimator described above is slightly less restrictive in this case and will be consistent under weaker conditions. However, as pointed out by Wooldridge (1997), the gains to consistency from using a control function estimator may not be worth the extra computation required, since the standard errors will need to be adjusted to reflect the two-stage estimation process.

3.3 Data

3.3.1 Background

This study utilizes a unique Finnish dataset of 37,000 young men who were performing their military service in 1982. Since military service is compulsory in Finland, the sample contains almost the entire 1962 birth cohort.[7] This dataset includes scores from a battery of ability tests performed during service. Statistics Finland has linked the army sample to a longitudinal file of Finnish censuses which contains usual labor market information. The latest information available on earnings and schooling is from 1994 administrative labor market records. Family links are provided in the Finnish censuses, so the dataset also contains labor market information on the parents of the men in the sample. Below several features of the dataset are discussed in greater detail.

[7] It is possible to postpone military service because of school enrollment and apply for service earlier as a volunteer. For this reason only about 70% of the sample were actually born exactly in 1962.

Test scores

The Finnish Defense Forces Basic Ability Test (Peruskoe 1) developed by the Finnish Defense Forces Education Development Center has been administered since 1955 for all new recruits at the beginning of their service. The ability test consists of three subtests measuring verbal ability, analytical reasoning and mathematical reasoning.[8] The raw scores of different subtests are approximately normally distributed and range from 1 to 40. The army uses a combined and scaled score in the selection of the rookies that are given officer training. In this paper the scores are handled separately and are standardized to a mean of zero and unit variance. Because the tests may partially reflect schooling attained, this analysis uses the residual component of test scores that is not explained by education level at time of test. We obtained these residuals by regressing the test scores on pre-test schooling, final schooling level and family background variables. Then we subtracted from the test scores the component that was explained by pre-test schooling. The component explained by post-test schooling was left in, since it obviously could not influence the test scores.

Educational information

Schooling information in the Finnish data is based on the Register of Degrees and Examinations compiled by Statistics Finland. The register is updated yearly with information submitted directly by educational institutions. The data contains a five-digit code in which the first digit indicates the level of education. For this analysis, degrees completed are converted to years of schooling based on the Standard Classification of Education by Statistics Finland.[9] Since the data is based on administrative reports it is free from the recall errors common in survey data. The major problem with the data is that it has no record of schooling that did not lead to a degree.

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[8] A more detailed description of the test items can be found in Uusitalo (1996).

[9] Statistics Finland classifies post-compulsory education into six levels according to the length of education. Here individuals with no record on post-compulsory education are assigned to the lowest level (9 years) of schooling. For this analysis, an additional category was created splitting the highest level into licentiates and PhD's. All other levels of education are coded according to the official classification, resulting in eight possible levels of education corresponding to 9, 11, 12, 14, 15, 17, 19 and 21 years of schooling.

Earnings information

The earnings information in Finnish census data is based on income tax reports and is collected directly from tax authorities. In this paper, annual pre-tax income from wages and salaries are used as the earnings measure. As with education, this measure of earnings is better than usual in that it is free from the usual reporting and recall errors. Unfortunately, there is no reliable measure of hours worked so it is not possible to calculate an hourly wage. Instead, log monthly earnings (calculated using annual income and available information on months worked[10]) is used as the outcome variable.

Geographic information

To control for local labor market differences and other geographic factors that may influence earnings and schooling, some specifications utilize information on place of residence in 1980, when most of the men of the sample were 18 years old. We include indicators for geographic region, residence in a city that has a university, and population of city of residence in 1980.

[10] Months worked is calculated by subtracting months unemployed from months in the labor force. Unemployment data is based on unemployment records of Ministry of Labor and labor force participation on pension registers. There is some measurement error in the months worked (average monthly wage decreases with months worked). To minimize the problem, observations reported as having worked less than six months are excluded from the analysis.

3.3.2 Descriptive statistics

The analysis is restricted to men who are out of school and work full-time.[11] Also excluded are observations with reported monthly earnings below 2,000 marks and observations with missing family background variables (father's and mother's education). As a result of these restrictions, the effective sample is 22572 for most specifications. Table 1 presents some descriptive statistics of this sample. Comparing the Finnish data to more commonly used datasets with similar information content, such as the National Longitudinal Survey of Youth, it can be concluded that these data have several advantages. The sample size is greater than in comparable datasets. Reliance on administrative data ensures more accurate information on schooling and earnings. The ability test score data is of comparable quality. The main disadvantages of this dataset are its lack of information on hours and non-completed spells of education.

Table 1. Descriptive statistics

Variable	Mean	Std Dev	Minimum	Maximum
Earnings 1994	128742	57929	12000	1034721
Calculated monthly wage (1994)	11229	4763	2000	86899
Years of schooling 1994	12.04	2.38	9	21
Potential (Mincer) experience (1994)	13.18	2.33	3	25
Calculated work experience (1994)	11.32	4.35	0.5	25.58
Age in 1994	32.22	1.52	29	41
Residual component of age	0.009	1.394	-5.407	9.075
Logic test score (normalized)	0.116	0.987	-2.186	2.641
Verbal test score (normalized)	0.114	0.980	-2.782	2.312
Math test score (normalized)	0.113	0.966	-3.807	2.783
Mother's years of schooling (1980)	9.88	1.63	9	20
Father's years of schooling (1980)	10.11	2.09	9	20
Mother's earnings in 1980	27033	20032	0	283699
Father's earnings in 1980	46380	39195	0	618291
Population of city of residence in 1980	73968	128972	<1000	483000
Living in a university city in 1980	0.255	0.436	0	1

N=22572

[11] In 1994 Finland was going through the biggest recession of the century and the unemployment rate was 18%. Excluding the unemployed from the sample is likely to reduce the estimates for the return to schooling, since those with less schooling (and earnings) are more likely to be unemployed.

Table 2 presents results from several simple OLS regressions of log earnings on schooling. The estimated return to schooling of 0.091 in Column (1), a conventional earnings function using Mincer experience (age-schooling-7) as a proxy for true work experience, is consistent with previous estimates using Finnish data (Asplund 1993). The model in Column (2) is the same except a more accurate measure of potential experience is substituted for Mincer experience. This measure is the amount of time lapsed since the last degree was received. In Column (3) both measures are used, and they share the task of explaining wages pretty equally.

Columns (5) - (8) include measures of family background and ability test scores in addition to the simple Mincer specification. Including ability measures in the regression (Column (6)) reduces the return to schooling somewhat, from 0.085 in Column (4) to 0.079. All three ability measures affect earnings positively, although the effect of the verbal test score is insignificant. Of the three ability measures, the math test score has the largest direct impact on earnings. The inclusion of parents' education and earnings in the model (Columns (6) and (7)) decreases the estimated return to schooling only slightly, even though parents' earnings and father's schooling all have a clear positive effect on their sons' earnings. In Column (6), which includes parents' schooling levels but not earnings, father's schooling appears to have a significant positive effect on earnings. However, with the addition of parents' earnings and ability (Column 8), the effect of father's schooling is no longer significant and the effect of mother's schooling appears to be negative. Ability and parents' earnings retain their positive effect on wages in this specification.

Table 2. OLS estimates of the return to schooling

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Schooling	0.091 t=55.3	0.087 t=61.9	0.096 t=56.7	0.085 t=60.9	0.079 t=53.7	0.083 t=57.7	0.083 t=58.7	0.077 t=51.7
Mincer experience	0.029 t=17.4		0.018 t=9.5					
Calculated work experience		0.014 t=18.8	0.010 t=11.9	0.014 t=17.8	0.015 t=19.2	0.014 t=18.1	0.014 t=18.6	0.015 t=7.7
Math score					0.031 t=7.8			0.030 t=7.6
Verbal score					0.003 t=0.9			0.003 t=0.7
Logic score					0.013 t=3.8			0.011 t=3.3
Father's schooling						0.008 t=5.6		0.001 t=0.8
Mother's schooling						0.000 t=0.04		-0.005 t=-2.8
Father's earnings (10 000 FIM)							0.006 t=10.0	0.006 t=7.7
Mother's earnings (10 000 FIM)							0.007 t=5.93	0.007 t=5.3
Regional variables	no	no	no	yes	yes	yes	yes	yes
R squared	0.179	0.181	0.184	0.194	0.202	0.196	0.199	0.206

N=22572. T-statistics reported below coefficients.

3.4 Instrumental Variables and Control Function Estimation

3.4.1 Selection of Instruments

The OLS estimates in the previous section will be inconsistent as long as (a) earnings are affected by other unobservable factors which are correlated with schooling or (b) the return to schooling itself varies and individuals at least partially observe their own returns. Under conventional assumptions, IV estimates will be consistent under (a) but not (b). In this case, consistency of IV is dependent on stronger assumptions. A control function estimator that explicitly accounts for the variation in return to schooling will be consistent under both (a) and (b) under slightly weaker conditions. Both estimators require an instrument that exerts no independent effect on earnings.

A good instrument must be highly correlated with the endogenous variable of interest (condition IV1) and not be correlated with the error term in the outcome equation (condition IV2). Two possible instruments available in the data are residence in a university city in 1980 and parents' education. Residence in a university city is similar to other instruments that have been used in the literature (see, for example, Card 1995b) and is generally considered to be a credible instrument. Its appeal is that as long as other regional differences are controlled for, it seems unlikely to affect earnings except through its effect on schooling. As for parents' schooling, instruments based on family background are less credible in general because they are likely to be correlated with unobservable determinants of earnings. Table 2 shows that controlling for parents' earnings, parents' schooling has no direct effect on earnings, but this does not prove that it meets condition IV2.

Both potential instruments for schooling satisfy condition IV1, and this can be verified easily. Results from two sets of reduced form schooling regressions are presented in Table 3. Note that since these regressions are the first stage of two stage least squares, all exogenous variables from the earnings equation are included in the schooling equations and all endogenous variables must be excluded. Naturally, any measure of experience which is based on schooling will also be endogenous, so an instrument for experience is needed also. Age is the usual candidate, but in this case age is slightly endogenous with respect to schooling, due to selection into the sample.[12] So in this analysis the instrument for experience is the residual component of age with respect to schooling: the conditional means E(Age|Schooling) are computed at each schooling level and are subtracted from actual age. Since the university city instrument is only likely to be valid if other geographic characteristics are controlled for, a full set of regional indicators is also included as well as the population of the city of residence in 1980.

Looking at the first column of the first table, the family background and ability test scores are, as expected, the most important determinants of schooling. Both mother's and father's schooling have a highly significant positive effect on son's schooling. An increase in either mother's or father's schooling by one year is associated with approximately 0.1 year increase in son's schooling, even controlling for ability differences. Growing up in a city with a university also increases the amount of education attained, although this effect is slight compared to the others. The age residual also has only a slight effect, as it should. The next two columns contain the first stage regressions for the two different kinds of experience. In both cases, the age residual is a powerful determinant of experience, while all the predictors of schooling have the expected negative effects on experience.

So, both parents' schooling and university city have significant effects on schooling, meeting condition IV1, but parents' schooling has greater explanatory power. This leads to somewhat of a tradeoff, since university city is probably more likely to meet condition IV2. In identifying potential biases in IV estimates, condition IV2 is typically emphasized more than condition IV1. Theoretically, the bias associated with a weak instrument that is just slightly correlated with the error term (as it always will be in finite samples) could be greater than the bias associated with a stronger instrument that is somewhat more correlated with the error term (see Bound et al. 1995). On the other hand, Card (1998) shows that using family background measures as instruments will lead to upward biased estimates.

A separate problem in instrument selection is that, if the returns are not constant, the estimates do not necessarily reflect the average effects in the population. As shown in Angrist

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[12] The sample consists of men who served in the army 1982. About two thirds of the men were 20 years of age at the beginning of their service. However, it is possible to postpone service or apply earlier as a volunteer. The most common reason for postponing service is continued schooling. Also, serving at 19 is very convenient for college-bound high school graduates. Therefore, both older and younger cohorts had on average more schooling before entering the army than the 1962 birth cohort.

and Imbens (1995), the instrumental variables estimate is a weighted average of causal responses to treatment (schooling) for those whose treatment status is affected by the instrument. They point out that in the well-known study by Angrist and Krueger (1991), the group with 8-12 years of schooling contributes the most to the estimates. In a related argument, Card (1995) shows that college proximity has larger effects on those with low predicted schooling. In this case IV estimates of the return to schooling based on college proximity will be upward biased since they get disproportionately more weight from students from poorer family backgrounds who face higher discount rates and, in optimum, higher marginal returns to schooling. This implies that the use of university city as an instrument will actually estimate the marginal returns education to those on the margin rather than the average return to a year of education. Here, we follow Card's suggestion and include the interaction of college city with predicted schooling as an instrument which permit the effect of university city to vary with predicted schooling.

Table 3. First stage regressions for 2SLS and control function estimation

University city as an instrument

Dependent variable	Years of schooling		Mincer experience		Calculated experience	
		t stat.		t stat.		t stat.
Age (residual)	0.029	3.07	0.976	130.58	0.933	53.01
University city (1980)	0.234	5.32	-0.158	-4.57	-0.214	-2.63
Father's schooling	0.195	25.60	-0.147	-24.52	-0.247	-17.53
Mother's schooling	0.180	18.74	-0.133	-17.71	-0.213	-12.00
Math score	0.711	32.98	-0.566	-33.43	-0.934	-23.43
Verbal score	0.416	19.72	-0.343	-20.72	-0.727	-18.65
Logic score	0.080	4.15	-0.075	-4.92	-0.239	-6.68
City population (in thousands)	-0.0006	-3.36	0.0006	4.05	0.0010	2.99

Table 3. cont. First stage regressions for 2SLS and control function estimation
University city and it's interactions with parents' schooling as an instrument

Dependent variable	Years of schooling		Mincer experience		Calculated experience	
		t stat.		t stat.		t stat.
Age (residual)	0.029	3.03	0.977	130.60	0.933	52.99
University city (1980)	-0.359	-1.98	0.113	0.79	-0.989	-2.94
Father's schooling	0.188	19.13	-0.144	-18.65	-0.269	-14.79
Mother's schooling	0.162	13.33	-0.125	-13.07	-0.229	-9.93
University city * mother's schooling	0.046	2.32	-0.022	-1.43	0.025	0.68
University city * father's schooling	0.013	0.87	-0.005	-0.40	0.051	1.80
Math score	0.712	33.02	-0.566	-33.44	-0.933	-23.43
Verbal score	0.416	19.74	-0.343	-20.73	-0.726	-18.62
Logic score	0.081	4.19	-0.075	-4.94	-0.238	-6.64
City population (in thousands)	-0.0006	-3.41	0.0006	4.08	0.0010	2.96

N=22572. T-statistcs are to the right.

3.4.2 IV and Control Function Estimates of the Return to Schooling

Table 4 contains, for each of the four instruments discussed above, a set of estimates of the return to schooling under four different IV and control function specifications, as well as OLS estimates for comparison. Results are presented using the two different measures of experience. All specifications include regional dummies and city population for the city of residence in 1980.

Row 2a contains estimates obtained using parents' schooling as an instrument. These estimates range from 0.098 to 0.118 depending on the specification, significantly exceeding their OLS counterparts in Row 1. The underlying assumption here, condition IV2, is that parents' schooling only affects sons' earnings through its effect on sons' schooling. This assumption is actually testable with over-identification tests since parents' schooling is actually two instruments (mother's schooling and father's schooling). The p-values from over-id tests are presented beneath the estimated standard errors. For both specifications, the hypothesis that condition IV2 is met is strongly rejected at the 5% level, so parents' schooling is not a valid instrument.

Table 4. Instrumental variables (2SLS) and control function estimates of the return to schooling

	(1)	(2)	(3)	(4)
Included regressors:	Mincer experience = Age – years of schooling -7		Experience calculated based on graduation dates	
Ability	yes	yes	yes	Yes
Parents' schooling	no	yes	no	Yes
1) OLS	0.083 (0.002)	0.082 (0.002)	0.079 (0.001)	0.077 (0.001)
2a) 2SLS (parents' schooling)	0.098 (0.004) ***p=0.006***		0.112 (0.005) ***p=0.005***	
2b) 2SLS (college city)	0.095 (0.018)	0.089 (0.034)	0.105 (0.018)	0.096 (0.033)
2c) 2SLS (college city and college city * parents' schooling)	0.090 (0.006) *p=0.111*	0.061 (0.028) *p=0.266*	0.098 (0.006) *p=0.085*	0.055 (0.029) ***p=0.042***
2d) 2SLS (college city and college city * predicted schooling)	0.092 (0.014) *p=0.816*	0.072 (0.024) *p=0.442*	0.091 (0.014) *p=0.212*	0.055 (0.025) *p=0.062*
3a) Control function (parents' schooling)	0.099 (0.004) $t_{S\eta}$=*-0.117*		0.111 (0.005) **$t_{S\eta}$ =*3.21***	
3b) Control function (college city)	0.095 (0.022) $t_{S\eta}$=*-0.134*	0.090 (0.033) $t_{S\eta}$=*-0.136*	0.100 (0.018) **$t_{S\eta}$ =*3.33***	0.091 (0.033) **$t_{S\eta}$ =*3.23***
3c) CF (college city and college city * parents' schooling)	0.090 (0.006) $t_{S\eta}$=*-0.460*	0.061 (0.028) $t_{S\eta}$=*-0.284*	0.098 (0.006) **$t_{S\eta}$ =*3.02***	0.052 (0.028) **$t_{S\eta}$ =*3.21***
3d) CF (college city and college city * predicted schooling)	0.092 (0.014) $t_{S\eta}$=*-0.117*	0.072 (0.024) $t_{S\eta}$=*-0.232*	0.088 (0.014) **$t_{S\eta}$ =*3.33***	0.053 (0.024) **$t_{S\eta}$ =*3.21***

N=22572. Dependent variable is 1994 log monthly wages. Excluded variables (instruments) in parentheses in left column. All regressions include experience as an endogenous right-hand side variable and use age as its instrument, and all regressions include as exogenous regressors a set of regional indicators and population of city of residence in 1980. First stage regressions for control functions are identical to the first stage regressions for the corresponding 2SLS model. The first stage regressions presented in Table 3 correspond to the 2SLS and control function estimates in models 2b, 2c, 3b, and 3c in Columns 4 and 8.

Standard errors are presented in parentheses below estimates. All standard errors on control function estimates are corrected for the presence of estimated regressors and heteroskedasticity of known form (see footnote 5). For 2SLS estimates, the values in italics below the standard errors are p-values from over-id tests. For control function estimates, the values in italics are t-statistics for the coefficient on $S\eta$.

Row 2b contains estimates obtained by using an indicator for residence in a university city in 1980 as an instrument. The schooling coefficients range from 9 to 11%. All IV estimates in this row exceed the corresponding OLS estimates in Row 1, but none by more than a standard error mostly because precision of the estimates using college city as an instrument is much lower than precision of the estimates obtained in Row 2a. Over-identification tests are not possible for these regressions since only a single instrument is used for schooling. However, the over-id tests for the next two rows lend credibility to university city as an instrument. The model in Row 2c uses university city and its interactions with parents' schooling as instruments, providing two over-identifying restrictions. For three of the four specifications in Row 2c, the test fails to reject these restrictions. The model in Row 2d includes the interaction between university city and predicted schooling as an instrument (in addition to university city) providing one over-identifying restriction. This restriction is not rejected.

The models in Rows 2c and 2d are particularly appealing in that they allow the instrument to have different effects for individuals with different characteristics. This avoids the bias associated with the instrument affecting different groups differently and adds precision to the estimates. Allowing an interaction between university city and parents' schooling allows for the possibility that the lowered costs associated with university proximity are more important to individuals from less-educated families, which seems plausible. In this model (Row 2c), the estimated returns are generally lower than in the previous two models (Rows 2a and 2b), particularly when the specification includes family measures. When both ability and parents' schooling are included (Column (4)), the estimates in Row 2c are lower than the corresponding OLS estimates. The pattern is similar in Row 2d, where, in addition to university city, the interaction between university city and predicted schooling is used as an instrument.

There is some variation in the three valid models (Rows 2b-2d), but overall, the estimates seem robust and similar to IV estimates in the literature. The estimates in Row 2b (where university city is the only instrument) are higher than the estimates in rows 2c and 2d because they assign more weight to those who are more likely to be affected by the instrument. In this case, university proximity is likely to affect only decisions about university (rather than other forms of education), so the estimates will reflect the average return to a year of university, which may be higher than the average return to schooling in general.

The lower section of Table 4 contains comparable results obtained using the control function approach described in Section 3.2. These results are, without exception, very close to the IV estimates. This is not surprising, since if the assumptions for IV are met, both methods will be consistent. Because a control function estimator is simply an IV estimate with an extra interaction term, a test of the extra restrictions required for IV is available: if the coefficient on the extra correction term ($S\eta$) is significant, the restriction doesn't hold and control function estimation is needed. If it is not significant, than there is no gain to using a control function estimator and IV is more efficient. The t-statistics for $S\eta$ are presented below the estimates and standard errors for the control function estimates. Right away, it becomes apparent that these statistics are very consistent within specification, but are not necessarily robust across different specifications. When Mincer experience is used (Columns (1) and (2)), the extra term is always insignificant and there appears to be no reason to go beyond IV. However, when calculated experience is used (Columns (3) and (4)), there does seem to be a significant positive effect of $S\eta$. In these specifications, it appears that there is a significant selection effect due to heterogeneous returns.

3.4.3 Allowing the Returns to Schooling to Vary with Observable Characteristics

With mixed evidence for the presence of heterogeneous returns, there remains the question of whether these returns can be partially or fully explained by characteristics semi-observable to the econometrician, for instance, ability or family background. This part of the analysis will explore this question by interacting these characteristics with schooling in an attempt to explain the selection effects.

The model implicit in the above control function estimates is essentially a generalization of equations (5) and (8) from Section 3.2:

$$S_i^* = \frac{b_i - r_i}{k} \tag{15}$$

$$\ln y_i = a + b_i S_i + X_i\alpha + \varepsilon \tag{16}$$

To make the IV and control function models in Table 4 estimable, it was necessary to assume that the variable part in marginal costs is a linear function of a set of observable covariates Z and unobservable factors ν_r:

$$r_i = \bar{r} + Z_i\gamma + \nu_r \quad (17)$$

where X_i and Z_i are row vectors of characteristics; Z_i includes all characteristics in X_i and at least one characteristic excluded from X_i. The control function estimator includes a term ($\hat{\eta}$S) that proxies the part of b_i that varies with unobservable characteristics but a restriction implicit in this model is that b_i does not vary in observable characteristics:

$$b_i = \bar{b} + \nu_b. \quad (18)$$

This restriction can easily be relaxed by including interaction terms between schooling and other observable characteristics. This is consistent with the assumption that the variable part of marginal returns is a linear function of observable characteristics A, and unobservable characteristics ν_b:

$$b_i = \bar{b} + A_i\phi + \nu_b. \quad (19)$$

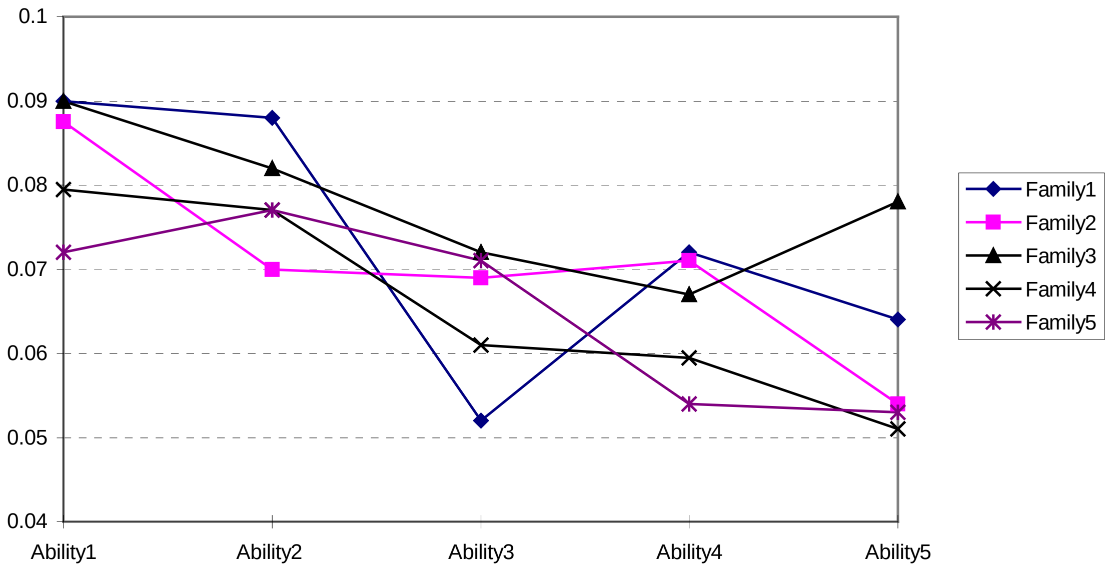
The schooling and earnings equations can then be written

$$S = \frac{1}{k}(\bar{b} - \bar{r} + A\varphi - Z\gamma) + \frac{1}{k}(\nu_b - \nu_r) \quad (20)$$

$$\ln(y) = a + (\bar{b} + A\varphi) \cdot S + \alpha X + (\varepsilon + \nu_b S)\,. \quad (21)$$

Specifying unnecessary interaction terms will reduce efficiency, but if b_i is related to observable characteristics, then it is much more informative to model this relationship directly instead of simply including an erroneous proxy. Figure 1 illustrates the relationship between several observable characteristics and the returns to schooling. For this figure, the sample is divided into cells based on ability quintile and family background quintile. Figure 1 plots the estimated schooling coefficients from an OLS regression of earnings on schooling, experience and experience squared estimated within these 25 cells. The pattern of the coefficients is clear: the estimated returns are higher at higher ability levels but do not vary much with parents' schooling. Hence, it would be informative to interact schooling with the ability measures since returns appear to vary with ability.

Mean returns to schooling in quintiles by family background and ability



Mean returns to schooling in quintiles by ability and family background

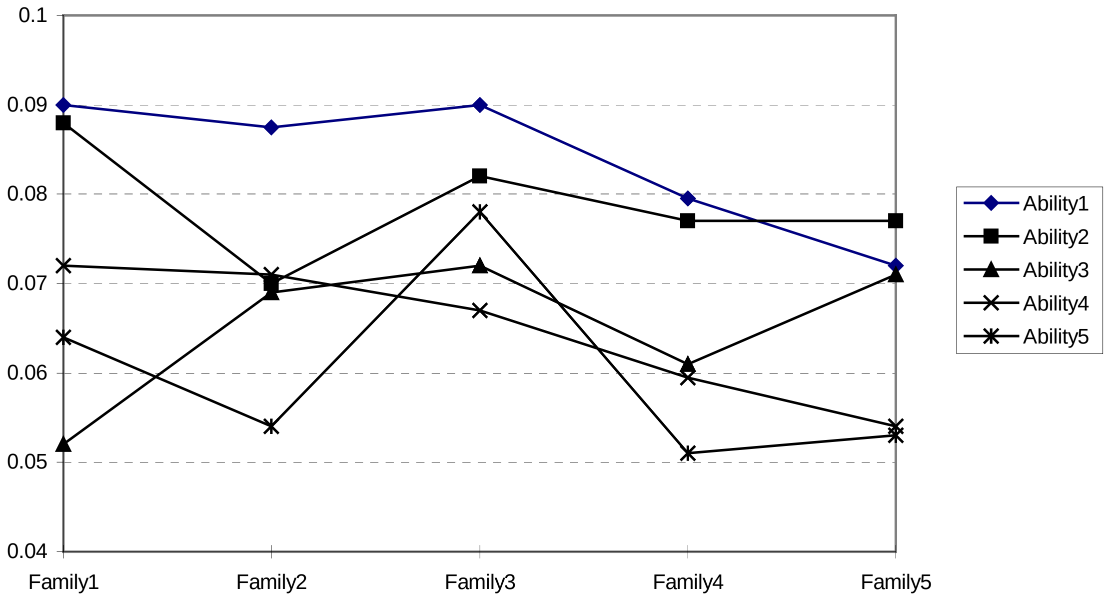


Figure 1

For this figure, the sample is divided into cells based on ability quintile and family background quintile, where "Ability1" denotes top quintile of ability, etc. Quintiles are defined by ranking individuals according to their predicted schooling based only on the three ability measures for ability quintiles and based only on parents' schooling for family background quintiles. Plotted above are the estimated schooling coefficients from an OLS regression of earnings on schooling, experience and experience squared estimated within these 25 combinations of ability and family background.

Table 5 contains a set of OLS and control function specifications, each estimated with and without interaction terms. Columns (1) and (2) contain the OLS results. In the first column, there is evidence that returns vary with ability since the interaction between schooling and the math test scores has a positive significant coefficient. This indicates that returns to schooling are on average higher for people with higher math ability. The pattern is not as clear for the other two ability measures. Verbal test score has no significant effect, and the logic test score has a significant negative effect, which would indicate that controlling for other factors, people with high logic scores gain less from schooling. Mother's schooling has a positive but insignificant effect on the return to schooling, while father's schooling has a significant negative effect. These patterns change when parents' earnings variables are included (Column 2). In this specification mother's schooling has a large positive effect, while fathers' schooling is insignificant. Father's earnings have no strong effect, but mother's earnings have a strong negative effect. The pattern for mothers' earnings and schooling is probably consistent with several different stories. The effects of ability remain unchanged from Column (1). These patterns persist when control function estimation is used, as can be seen in Columns (3) – (6).

The biggest difference between the OLS and the control function estimates is that with the latter, the return to schooling is very sensitive to the inclusion or exclusion of parents' earnings; it falls drastically when earnings are included in the specification. In the next section, this specification will be used, so it is important to note that the drop in returns to schooling is due to specification and not to something else. The purpose of adding interactions to the control function estimates is to see whether the selection due to heterogeneity can be explained with observable heterogeneity. In Columns (5) and (6), it appears that it can't – even though some of the interaction terms are significant, the coefficient on $S\hat{\eta}$ does not change with the inclusion or exclusion of interaction terms. So observable characteristics are important here, but there is also still significant unexplained variation in the returns to schooling.

Table 5. Estimates of the return to schooling with interactions

	(1)	(2)	(3)	(4)	(5)	(6)
Estimation method:	OLS		Control function using university city as an instrument			
Experience measure:	Calculated exper.		Mincer exper.		Calculated exper.	
Parents' earnings and interactions included:	No	yes	no	yes	No	yes
S (Schooling)	0.082 (0.006)	0.075 (0.006)	0.089 (0.034)	0.050 (0.036)	0.091 (0.033)	0.050 (0.036)
S*Math score	0.0043 (0.0016)	0.0044 (0.0016)	0.0048 (0.0016)	0.0049 (0.0017)	0.0046 (0.0016)	0.0048 (0.0016)
S*Verbal score	-0.0006 (0.0016)	-0.0004 (0.0016)	-0.0003 (0.0016)	-0.0001 (0.0016)	-0.0004 (0.0016)	-0.0002 (0.0016)
S*Logic score	-0.0037 (0.0015)	-0.0034 (0.0015)	-0.0035 (0.0015)	-0.0032 (0.0015)	-0.0033 (0.0015)	-0.0030 (0.0015)
S*Father's schooling / 100	-0.100 (0.048)	-0.079 (0.056)	-0.063 (0.049)	-0.046 (0.057)	-0.68 (0.048)	-0.048 (0.056)
S* Mother's schooling / 100	0.053 (0.060)	0.175 (0.069)	0.072 (0.061)	0.190 (0.068)	0.084 (0.060)	0.201 (0.067)
S*Father's earnings / 10000		-0.0004 (0.0003)		-0.0003 (0.0003)		-0.0003 (0.0003)
S*Mother's earnings / 10000		-0.0021 (0.0005)		-0.0020 (0.0004)		-0.0020 (0.0005)
Correction term for endog. experience			0.0416 (0.0164)	0.0374 (0.0164)	-0.0170 (0.0023)	-0.0189 (0.0023)
η (correction term for endog. Schooling)			0.0199 (0.0382)	0.0465 (0.0403)	-0.0397 (0.0329)	-0.0075 (0.0354)
S*η			0.0001 (0.0007)	0.0003 (0.0007)	0.0013 (0.0004)	0.0014 (0.0004)
<u>Same model as above estimated without interactions</u>						
S (Schooling)	0.077 (0.001)	0.077 (0.001)	0.090 (0.033)	0.063 (0.035)	0.091 (0.033)	0.063 (0.035)
Correction term for endog. experience			0.0497 (0.0135)	0.0464 (0.0134)	-0.0169 (0.0022)	-0.0188 (0.0023)
η			0.0299 (0.0367)	0.0522 (0.0389)	-0.0386 (0.0329)	-0.0120 (0.0354)
S*η			-0.0001 (0.0007)	0.0001 (0.0006)	0.0014 (0.0004)	0.0015 (0.0004)

Standard errors in parentheses below estimates All regressions include the main effects of the variables that are interacted with schooling as well as experience and regional variables. Standard errors on control function estimates are corrected for the presence of estimated regressors and heteroskedasticity of known form.

The information in Columns (3) – (6) allows examination of the pattern in the selectivity correction terms. When experience is calculated based on graduation date (Columns (5) and (6)), the coefficient on $\hat{\eta}$ is always negative and the coefficient on $\hat{\eta}S$ positive and significant, indicating that cost-based selection causes underestimation of the returns to schooling, while selection based on benefits naturally leads to overestimation of returns. Garen (1984) finds a similar pattern, which he attributes to comparative advantage in schooling. Lower than expected schooling is associated with higher than expected earnings at low levels of schooling. At higher levels of schooling the pattern is reversed; higher than expected schooling is associated with higher than expected earnings. When traditional Mincer experience is used, the coefficient on $\hat{\eta}$ is positive and the coefficient on $\hat{\eta}S$ is close to zero. This is consistent with traditional thinking about selection bias – that cost-based selection involves people with more financial backing (who would have done better anyway) receiving more schooling. However, the coefficient on $\hat{\eta}$ is not actually significant. It seems to be the coefficient on the selection term for experience that is driving the difference in the relationship between $S\hat{\eta}$ and wages.

3.5. Maximum likelihood estimation of the system

Implicit in the model which motivated the control function estimates in Table 5 (equations (20) and (21)) is a cross-equation restriction: note that the parameter vector ϕ is present in both equations. This restriction was not imposed in the estimation in Table 5, but is potentially testable if all parameters of the model can be identified. The model is not fully identifiable when the equations are estimated separately, but the system is actually overidentified when the equations are estimated simultaneously. In this section maximum likelihood estimation is used to jointly estimate the system of equations, allowing the above cross-equation restriction and other nested restrictions to be imposed and tested.

The joint normality assumed in the following maximum likelihood estimation is stronger than the requirements for IV or control function estimates stated in Section 3.2, but all of these conditions can be (and often are) generated by the assumption of joint normality. Although the IV and control function estimates presented above are consistent under weaker conditions, maximum likelihood estimation has several advantages in this case. Besides allowing the estimation and testing of the full model, MLE furnishes estimates of the full covariance structure of the system, providing inference about the extent to which the returns and costs vary and about their respective distributions.

Table 6 presents results from maximum likelihood estimation of the whole system. The specification in Column 1 is based on the model in equations (20) and (21). Full identification of this model rests on the assumption that ability affects schooling entirely through its effect on marginal benefits to schooling. This implies the restriction (later relaxed) that the coefficients on ability in the schooling equation must be equal to the coefficients of the interaction between ability and schooling in the earnings function. When these cross-equation restrictions are imposed, all the parameters of the model can be identified. In fact, the system is overidentified if at least two ability measures are available (with only one ability measure, k is still free), providing a test of the model.

Assuming that the unobservables in the schooling and earnings equations, ε, ν_b, and ν_r, covary freely and have a joint normal distribution,[13]

$$\begin{bmatrix} \nu_b \\ \nu_r \\ \varepsilon \end{bmatrix} \sim N\left(\begin{bmatrix} 0 \\ 0 \\ 0 \end{bmatrix}, \begin{bmatrix} \sigma_b^2 & \sigma_{br} & \sigma_{b\varepsilon} \\ \sigma_{br} & \sigma_r^2 & \sigma_{r\varepsilon} \\ \sigma_{b\varepsilon} & \sigma_{r\varepsilon} & \sigma_\varepsilon^2 \end{bmatrix}\right)$$

then for each level of schooling the error terms on the schooling and earnings equations are also normally distributed, with the following covariance structure:

$$\begin{bmatrix} \nu_b - \nu_r \\ \varepsilon + \nu_b S_i \end{bmatrix} \sim N\left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} \sigma_b^2 + \sigma_r^2 - 2\sigma_{br} & \\ \sigma_{b\varepsilon} - \sigma_{r\varepsilon} + (\sigma_b^2 - \sigma_{br})S_i & \sigma_\varepsilon^2 + \sigma_b^2 S_i{}^2 + 2\sigma_{b\varepsilon} S_i \end{bmatrix}\right).$$

Column (1) contains estimation results for the model described above. Column (2) estimates the same model but relaxes the constraint k_1=0 that returns to schooling do not vary with schooling. A likelihood ratio test fails to reject the restriction at any level of significance

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[13] In order to guarantee that the covariance matrix is positive definite the covariance matrix is specified as a product of a lower triangular matrix and its transpose. The optimization was done using the BHHH algorithm in GAUSS with analytical first derivatives. The estimated standard errors are based on a Hessian matrix calculated from outer product of the gradients at the optimum.

($\chi^2(1) = 0.05$), so it appears that in this sample, the relationship between schooling and log earnings is approximately linear even at the individual level.

In Column (3) the restriction that the ability measures have no effect on marginal returns is imposed and tested. This model is similar to the model implicit in the estimations of Table 4: returns to schooling are allowed to vary with unobservables but not observables. Comparing the log-likelihoods of Columns (1) and (3) this restriction is decisively rejected by a likelihood ratio test ($\chi^2(3) = 6804$). This is consistent with the pattern observed in Figure 1, where ability appears to have a strong effect on estimated returns. In Column (4) a restriction is imposed on the error structure to test the hypothesis that there is no random component in the marginal returns. This restriction, which allows schooling to vary only with observables, is also rejected ($\chi^2(3) = 131$). Apparently marginal returns to schooling are quite variable and are related to both observable and unobservable individual characteristics.

In Column (5) the cross-equation restrictions that constrain ability to affect schooling only through marginal returns are relaxed, allowing ability to affect schooling through marginal costs as well as benefits. In terms of the model this frees the coefficients on the ability variables in the schooling equation, since they are no longer constrained to be the same as the interaction coefficients in the earnings equation. This specification provides a test of the cross-equation restriction (implicit in the specification in Column (1)) that ability affects schooling only because it affects the return to schooling. These cross-equation restrictions implied are rejected ($\chi^2(2) = 14$), implying that ability must also influence schooling through its effect on costs of schooling.

The specification in Column (6) is similar to Column (5) except ability is restricted to affect the schooling decision only through marginal costs. This restriction is also rejected ($\chi^2(3) = 27$). It appears that the strong effect of ability on schooling attainment is due to the combined effect of lower costs and higher returns for individuals with higher ability. In Column (7) the constraints made in Column (1) are relaxed to allow family background (parents' schooling levels) to affect the rate of return to schooling as well as the discount rate. This time the restrictions of Column (1) are not rejected ($\chi^2(2) = 2.5$), implying that the strong effect of family background on schooling is due only to its effect on the discount rate. Again, this is consistent with the pattern shown earlier in Figure 1.

The estimated average return to schooling does not vary much with the different specifications of marginal returns and marginal costs. In all specifications the average return is close to 6%. This is smaller than the OLS estimates, but is very similar to the IV and control function estimates, particularly those with similar specifications. As predicted, math and verbal test scores have significant but small positive effects on the marginal return to schooling. Ceteris paribus, an individual with a math test score three standard deviations above the mean would have a return to schooling of only 0.0637 (as opposed to the mean return of 0.0547) according to the specification in Column (1). In Column (5), where the cross-equation restriction is relaxed and ability is allowed to affect schooling through costs as well as returns, the predicted variation in marginal returns is much larger. An individual with a math score three standard deviations above the mean would now have a return to schooling of 0.08.

Returns to schooling vary considerably between individuals. The point estimate for the standard deviation of the random coefficient is 0.020 in Columns (1) and (3). The mean return to schooling in Column (3) is 0.06, but if marginal returns to schooling are normally distributed, which is consistent with the structural assumption, then only 38.3% of individuals have marginal returns between 0.05 and 0.07. So there is a significant amount of dispersion in individual returns to education, but this dispersion is within practical limits, with a very reasonable range of returns to schooling. If returns are distributed normally, as assumed, then 98.8% of individual returns fall between 0.01 and 0.11, and only 0.1% of individuals have negative returns to schooling.

Table 6. Maximum likelihood estimates of the jointly estimated model

Variable	Parameter	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Marginal returns: $b_i = \bar{b} + \phi_i A_i + kS + \nu_b$								
Average rate of return to schooling	$\bar{b}$	0.0547	0.0579	0.0603	0.0622	0.0554	0.0593	0.0568
		(0.0063)	(0.0153)	(0.0038)	(0.0061)	(0.0063)	(0.0061)	(0.0105)
Slope of return (w.r.t schooling)	k_1		0.0008					
			(0.0036)					
A_1 (math score)	ϕ_1	0.0029	0.0032		0.0025	0.0082		0.0029
		(0.0008)	(0.0018)		(0.0008)	(0.0017)		(0.0008)
A_2 (logic score)	ϕ_2	-0.0001	-0.0001		-0.0001	-0.0043		-0.0001
		(0.0001)	(0.0001)		(0.0001)	(0.0016)		(0.0001)
A_3 (verbal score)	ϕ_3	0.0018	0.0021		0.0016	-0.0009		0.0019
		(0.0005)	(0.0012)		(0.0005)	(0.0017)		(0.0005)
F_1 (father's schooling level)	ϕ_4							0.0003
								(0.0004)
F_2 (mother's schooling level)	ϕ_5							-0.0006
								(0.0004)
Discount rates: $r_i + kS = \bar{r} + \delta F_i + kS + \nu_r$								
Average discount rate	$\bar{r}$	0.0437	0.0455	0.0494	0.0525	0.0445	0.0484	0.0455
		(0.0075)	(0.0108)	(0.0038)	(0.0073)	(0.0063)	(0.0061)	(0.0108)
Slope of discount rate (w.r.t schooling)	k_2	0.0036	0.0033	*=0.0036**	0.0032	*=0.0036**	*=0.0036**	0.0037
		(0.0001)	(0.0033)		(0.0010)			(0.0010)
F_1 (father's schooling level)	δ_1	-0.0006	-0.0007	-0.0010	-0.0005	-0.0006	-0.0006	-0.0003
		(0.0002)	(0.0004)	(0.0000)	(0.0002)	(0.0000)	(0.0000)	(0.0004)
F_2 (mother's schooling level)	δ_2	-0.0006	-0.0006	-0.0009	-0.0005	-0.0005	-0.0005	-0.0012
		(0.0002)	(0.0003)	(0.0000)	(0.0002)	(0.0000)	(0.0000)	(0.0001)
$A_!$ (math score)	δ_3					0.0053	-0.0028	
						(0.0017)	(0.0001)	
A_2 (logic score)	δ_4					-0.0043	0.0001	
						(0.0016)	(0.0001)	
A_3 (verbal score)	δ_5					-0.0027	-0.0018	
						(0.0017)	(0.0001)	

Table 6 continued

Variable	parameter	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Earnings equation: αX								
A_1	α_1	0.0269	0.0253	0.0306	0.0313	0.0114	0.0332	0.0252
(math score)		(0.0065)	(0.0096)	(0.0039)	(0.0065)	(0.0079)	(0.0063)	(0.0096)
A_2	α_2	0.0115	0.0115	0.0113	0.0102	0.0228	0.0107	0.0115
(logic score)		(0.0034)	(0.0034)	(0.0034)	(0.0034)	(0.0054)	(0.0034)	(0.0034)
A_3	α_3	0.0040	0.0029	0.0063	0.0068	0.0115	0.0081	0.0029
(verbal score)		(0.0051)	(0.0068)	(0.0037)	(0.0050)	(0.0068)	(0.0049)	(0.0068)
Father's earnings	α_4	0.0008	0.0008	0.0008	0.0007	0.0008	0.0008	0.0007
(in 10,000 FIM)		(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Mother's earnings	α_5	0.0009	0.0009	0.0009	0.0009	0.0009	0.0009	0.0010
(in 10,000 FIM)		(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Experience	α_6	0.0362	0.0362	0.0356	0.0322	0.0356	0.0346	0.0362
		(0.0029)	(0.0029)	(0.0029)	(0.0026)	(0.0029)	(0.0028)	(0.0029)
Experience^2	α_7	-0.0012	-0.0012	-0.0011	-0.0009	-0.0012	-0.0011	-0.0012
		(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Intercept on	a	8.7622	8.7569	8.7469	8.7522	8.7635	8.7538	8.7563
earnings		(0.0222)	(0.0322)	(0.0182)	(0.0215)	(0.0222)	(0.0221)	(0.0322)
Covariance matrix of the error terms								
Var(νb)	σ_b^2	0.000420	0.000428	0.000424		0.000404	0.000408	0.000424
Cov(νb,νr)	σ_{br}	0.000371	0.000368	0.000363		0.000358	0.000360	0.000375
Cov(νb,ε)	σ_{b_ε}	0.000656	0.000625	0.000581		0.000667	0.000654	0.000620
Var(νr)	σ_r^2	0.000370	0.000369	0.000365	0.000037	0.000358	0.000358	0.000375
Cov(νr,ε)	σ_{r_ε}	0.000719	0.000724	0.000749	-0.000112	0.000738	0.000739	0.000710
Var(ε)	σ_ε^2	0.115595	0.115692	0.115932	0.126009	0.115600	0.115866	0.115683
Log-likelihood		-0.603265	-0.603264	-0.752867	-0.606151	-0.602957	-0.603546	-0.603210

N=22739 *not identified, fixed at the same value as in the first column

In order to guarantee that the covariance matrix is positive definite we actually specify the covariance matrix as a product of a lower triangular matrix and its transpose and estimate the elements of this triangular matrix. In the estimation we used the BHHH algorithm in GAUSS and provided analytical first derivatives. The estimated standard errors are based on a Hessian matrix calculated from outer product of the gradients at the optimum.

3.6 Conclusion

In this paper we have specified a model of schooling choices that explicitly accounts for individual variation in the returns and costs of schooling. We have also extended the analysis of heterogeneous treatment effects to the case of continuous treatments and shown that, contrary to a discrete choice case, the average effects of schooling on earnings can still be consistently estimated with traditional instrumental variables methods, given slightly stronger assumptions than usual. However, a simple control function estimator is available that is consistent under weaker assumptions.

We compare IV and control function estimates under a set of different specifications, and find that while the control function estimates are consistently lower than their IV counterparts, this difference is never significant. We obtain similar results with maximum likelihood estimation. These results show a considerable variation in individual returns, only part of which is captured by the interactions between schooling and the observable individual characteristics. These results suggest that family background mainly influences schooling choices through its effect on the costs of schooling while ability affects schooling choice through both costs and benefits. The observable part of variation in the returns to schooling could reflect individual differences in ability to convert the human capital absorbed in school into marketable skills valued at the workplace.

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Chapter 4

Schooling choices and the return to skills

Abstract

I estimate returns to education in a discrete choice model, where individuals have several potentially non-independent alternative options. An ordered generalized extreme value model is used for describing the choice problem and returns to schooling are calculated by comparing actual and counterfactual outcomes.

The empirical analysis is based on data on personality test scores from the Finnish Army files. Various personal characteristics, and family background, are shown to have a large impact for the schooling choice but a relatively small effect on earnings.

Keywords: OGEV-model, return to skills, selectivity

4.1 The nature of the problem

The human capital theory of educational choice involves optimizing agents deciding on investments in additional schooling based on the marginal costs and the (expected) marginal benefits of extra years of schooling. Human capital investments will be continued as long as the marginal benefits exceed the marginal costs. However, the marginalist approach is rather awkward in a more realistic case where the agents face a choice between discrete alternatives. The eventual level of schooling is determined by a sequence of such choices. Nevertheless, these individual choices are not marginal and usually involve major investments that take several years to complete.

Also, the upgrading of the educational system tends to happen not by extending the length of education at current schools, but by accepting more students to schools providing higher education. Since the mid 1980's the number of new students in the Finnish universities have risen by approximately 40% and the creation of new higher education institutions, "polytechnics", will further increase the number of students in higher education. A discrete choice framework seems better suited for the evaluation of potential benefits of such policies.

In the Finnish schooling system, there are two major selection points. The first occurs after nine years of compulsory schooling when approximately half of the students continue in upper secondary schools. These schools provide three years of general education, at the end of which students take "the matriculation examination" that is generally required for university admission. The other half continue in various vocational schools. The second major selection stage, which is the focus of this paper, occurs after secondary schooling. Of those completing general upper secondary education around one third are admitted to universities while the remainder continue their studies in vocational education institutions or move to the labor market.

The goal of this paper is to estimate the returns to education in a setting where individuals are faced with a choice between several alternative education levels. The interesting question is how individuals, whose labor market outcomes are observed after their educational choices, would have succeeded, had they chosen another education career. In the program evaluation jargon the interest is in "the treatment effects on the treated" as well as in "potential treatment effects on the non-treated" (Heckman, 1997). Evaluating such counterfactual outcomes naturally requires some assumptions on the choice mechanism, since alternative outcomes can never be observed.

There are two central issues in computing the counterfactual outcomes (Vella 1996). First, the labor markets at the different levels of schooling may value individual characteristics differently. The skills that are useful in jobs typically filled with college graduates may differ greatly from skills that are rewarded in jobs that are typically filled with high school graduates. Therefore, skill prices may vary across the educational categories.

A number of studies use scores in cognitive skill tests as measures of relevant personal characteristics (e.g. Murnane, Willet and Levy 1995; Grogger and Eide 1995; Blackburn and Neumark 1993). Especially mathematical ability has been shown to have an effect on the labor market outcomes. However, cognitive skills are not the only measurable skills that are valued at the market. As most jobs require interacting with others, social skills, communication skills and motivation are also important. A clear indication of their importance is that the employers are willing to spend considerable sums of money to find an employee with a particular skill mix. Testing the job applicants using some sort of personality tests is a widespread practice, especially in the white-collar jobs. However, there is very little evidence of the effect of non-cognitive skills on earnings. The issue is difficult, foremost, because relevant skills are hard to identify and accurately measure. In this study a rather unique set of skill measures from ability and personality tests administered by the Finnish Army is used to empirically assess the importance of such factors.

The second issue is the endogeneity of the investment decision. Faced with different skill prices prevailing at the different labor markets, rational individuals choose the educational level leading to the occupations where their skills are best rewarded. The optimizing process causes self-selection so that only censored samples are available in each education level. Censoring must be somehow accounted for unbiased estimation of structural relationships from a censored sample. This can be achieved using various selectivity correction methods. However, optimization also reveals information about the unobservable components. If a certain choice was made by an optimizing agent, it must have maximized utility for the decision maker. This information can be exploited in calculating the counterfactual outcomes.

In this paper I first formulate a discrete choice schooling model in a way that is consistent with the random utility maximization assumption (McFadden 1981). In order to avoid imposing unrealistic assumptions on the independence of unobservable random components across the alternative options, I use an ordered generalized extreme value model by Small (1987). Then earnings functions on different levels of schooling are estimated using a simple selectivity correction method suggested by Lee (1982). The Lee method derives the selectivity correction terms as a function of choice probabilities alone. The attractive feature of the method is that it provides a straightforward way of calculating counterfactual outcomes (Lee 1995a).

In section 4.2 the econometric issues concerning the choice problem, selectivity correction and estimation of counterfactual outcomes are discussed. Section 4.3 describes the data that have been created by combining information of psychological test scores from the Finnish army with longitudinal census data files. Section 4.4 presents the estimation results and Section 4.5 concludes with some final comments.

4.2 Econometric issues

The first empirical application of polychotomous choice models with selectivity to the schooling choices and earnings was in an article by Trost and Lee (1984). They explain a choice between three possible schooling levels using a multinomial logit model. They then correct for potential selectivity bias in the earnings functions using methods in Lee (1983). The problem in their study is that the multinomial logit model is derived from the axiom of independence of irrelevant alternatives and, therefore, imposes a strong independence restriction on the unobserved utility components. As Vella and Gregory (1996) point out, this assumption is likely to be violated in the estimation of educational outcomes. Some unobserved factor may make all levels of education that require a long period of study more or less likely to be chosen. In an attempt to avoid the independence assumption, Vella and Gregory (1996) use an ordered probit model instead. An ordered response model specifies educational choices in terms of a single latent variable and the observed choices reflect intervals on this one-dimensional index. However, the assumption that choices depend on only a single index is also quite restrictive and may lead to serious biases in the case that the true model is unordered (Amemiya 1985). Since the models are not nested, it is difficult to find a data-based selection criterion between the two.[1]

To avoid imposing implausible a priori assumptions on the correlation pattern on the unobservable components, I chose a more general approach. Small (1987, 1994) presents an ordered generalized extreme value (OGEV) model[2] which relaxes the multinomial logit independence assumptions and allows correlation between unobservable components of adjacent schooling levels but still retains the flexibility and ease of estimation of multinomial logit models.[3]

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[1] Some attempts to formulate an ordered model that is nested in an unordered model can be found in the Statistics literature. Andersson (1984) discusses "a stereotype model" that is closely related to the ordered logit model. The stereotype model imposes restrictions in the multinomial logit coefficients resulting in a model where coefficients of different alternatives are proportional and, therefore, allows testing of the single-dimensionality assumption.

[2] Despite its apparent plausibility the OGEV-model has not been used in empirical studies. Small (1987) applies the model to study trip scheduling by automobile commuters facing congestion and establishes computational feasibility but does not find strong support for the model's applicability.

[3] Another possibility would be to model choices using a multinomial probit model that would allow any correlation patters between the random utility components. As this would require evaluating multivariate integrals, the computational burden would be far greater, though with only four

As a starting point let us suppose that there is one potential regression outcome in each schooling level

$$y_{is} = x_i \beta_s + u_{is}, \quad i = 1, \ldots, N, \quad s = 1, \ldots S, \tag{1}$$

where x_i is a vector of individual characteristics and β_s a sector specific vector of skill prices prevailing in labor markets for those with schooling level *s*. Random components in individual wages are captured with a stochastic term u_{is} that has a zero mean in the population. The econometric problem arises because the *s*th equation can only be estimated from the data on those with schooling level *s*. If the choice of schooling depends on the expected outcomes, the expected value of u_{is}, conditional on the observed level of schooling, is not zero. Furthermore, if the variables that are included in the regression equation also have an effect on the choice probability, they will be correlated with the error term inducing biases in the estimates. However, given suitable assumptions on the choice mechanism, the conditional expectation of u_{is} can be added to the equation (1) and consistent estimates of parameters β_s can be obtained with ordinary least squares regression.

4.2.1 Ordered generalized extreme value model

Let the utilities of different schooling levels for individual *i* be

$$U_{is} = V_{is} + \varepsilon_{is} = z_i \gamma_s + \varepsilon_{is}, \tag{2}$$

where $s = 1,...,S$ indexes the alternatives and $i=1,...,N$ individuals. V_{is} are non-stochastic "strict utilities" that depend on a vector of individual characteristics z_i and alternative specific coefficient vectors γ_s. To simplify the notation the individual subscript is suppressed below. The random components ε_{is} have a joint generalized extreme value distribution.

$$F\left(\{\varepsilon_s\}\right) = \exp\left[-G\left\{e^{-\varepsilon_s}\right\}\right], \tag{3}$$

where $G(y_1,...,y_S)$ satisfies the conditions for random utility maximization in McFadden (1981). Equation (2) can be viewed as a reduced form of a structural equation that has present

alternatives, still feasible. In addition to computational simplicity, the OGEV- model also allows testing restrictions that simplify the model to a multinomial logit model.

value of expected lifetime earnings as one of its components. Supposing that individuals choose the alternative that maximizes their utility, it then follows that the probability that alternative k is chosen is

$$P_k = \frac{\exp(V_k) G_k}{G} = \frac{\exp(V_k + \log G_k)}{\sum_{s=1}^{S} \exp(V_s + \log G_s)}, \tag{4}$$

where G_j denotes the derivative of G with respect to the jth argument. In the simplest case $G = \sum y_s$ and the above equation reduces to the multinomial logit model. To allow for a more flexible covariance structure in $\{\varepsilon_s\}$ Small (1987, 1994) suggested defining G as follows

$$G = \sum_{r=1}^{S+M} \left[\sum_{s \in B_r} w_{r-s} y_s^{1/\rho_r} \right]^{\rho_r}, \tag{5}$$

where M is a positive integer related to the number of alternatives in same subset; w_m, m = 0,...M are non-negative weights that sum up to one; ρ_r is a parameter describing the correlation among close alternatives and

$$B_r = \{ s \in \{1, \dots, S\} | r - M \le s \le M \}$$

define the nesting structure.

The model closely resembles the nested logit model (McFadden 1981) except that the subsets B_r overlap. For example, with M = 1 and J = 4 the subsets are $B_1 = \{1\}, B_2 = \{1,2\}, B_3 = \{2,3\}, B_4 = \{3,4\}$ *and* $B_5 = \{4\}$. As in the nested logit model, the stochastic elements in the same subset can be correlated with each other. Letting the subsets overlap allows correlation between the alternatives that are close in some natural ordering.

For practical estimation purposes, it is necessary to reduce the number of free parameters in (5). Small (1987) suggests restricting the weights be equal w_m = 1/(M+1) for all m and the within nest covariance parameter $\rho_r = \rho$ for all r. This leads to the "standard OGEV model". The model can be estimated with maximum likelihood but it is simpler to use a first order approximation and specify the OGEV-model using pseudovariables and to estimate probabilities of the form

$$P_k = \frac{\exp\left(V_k + \theta N_k\right)}{\sum_{s=1}^{S} \exp\left(V_s + \theta N_s\right)}, \tag{6}$$

where

$$N_s = w_{r-j} \log \sum_{j \in B_r} w_{r-j} \left(P_j^0 / P_s^0 \right) \tag{7}$$

and P^0 are predicted probabilities from the multinomial logit model. Now, the model can be estimated in two steps both involving only computationally simple logit models.[4] The pseudovariable N_s captures the effect of close substitutes. In fact, contrary to usual two-step estimators, the pseudovariable estimator also provides consistent estimates of the standard errors of the parameters since all the parameters are re-estimated in the second stage (Small 1987).

4.2.2 Selectivity correction

Given the estimated choice probabilities from the OGEV-model, selectivity correction can be done as suggested by Lee (1983).[5] The Lee correction constructs the expected values of the random terms in the outcome equations as functions of the choice probabilities. Most

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[4] Note that in the multinomial logit model the coefficients vary across alternatives while the variables z_i are constant for each individual. In equation (6) the pseudovariable gets different values for each alternative but a single coefficient as in the conditional logit model (Maddala, 1983). The simplest way to estimate the combined model is to define a set of alternative specific dummy variables and let the individual characteristics enter as interactions with these dummy variables. Then the model can be estimated using software for conditional logit models.

[5] Hay (1980) and Dubin and McFadden (1984) propose an alternative selectivity correction method. While the Lee approach is based on transformation of random utility components through maximum order statistics, the Hay approach generalizes the Heckman two-step method directly to a multivariate setting. As Schmertmann (1994) notes, the Hay approach is slightly less restrictive in its assumptions, but more prone to multicollinearity problems caused by correlation between the original regressors and synthetic regressors (correction terms). An attempt to apply the Hay approach to the data in this study did not yield stable results. Also semiparametric selectivity correction methods for polychotomous choice models have been developed recently (Ichimura and Lee 1991, Lee 1995b) but not much is known of their performance in empirical applications.

applications have generated these predicted probabilities using the multinomial logit model, but there is no reason why a more general first stage approach could not be used.[6]

For the Lee specification, it is useful to define a new random variable η

$$\eta_s = \left\{\max V_j : j = 1, \ldots S, j \neq s\right\} - \varepsilon_s . \tag{8}$$

The alternative *s* is then chosen if

$$V_s > V_j ,\, j = 1, \ldots S, j \neq s \Leftrightarrow \eta_s < \gamma_s z_i . \tag{9}$$

This new random variable is then transformed to a normal variate using a transformation $J = \Phi^{-1} F_s$, where F_s is the distribution function of η_s and Φ^{-1} the inverse normal distribution function. Clearly, the function F_s is nothing more than the probability of choice *s*. Assuming a joint normal distribution[7] for the transformed variable $\eta_s^* = J(\eta_s)$ and the error term u_s in the outcome (wage) equation, the expected value of the outcome equation error can be written as

$$\begin{aligned} E\left(u_s \middle| x, s\ chosen\right) &= \sigma_{u_s,\eta_s^*} E\left(u_s \middle| x, \eta_s^* < J(\gamma_s z)\right) \\ &= \sigma_{u_s,\eta_s^*} \frac{-\phi\left(\Phi^{-1}(F_s)\right)}{F_s} = \sigma_{u_s,\eta_s^*} \lambda(P_s) \end{aligned} , \tag{10}$$

where $\phi(.)$ denotes the standard normal density. $\lambda(P_s)$ is the standard selectivity correction term that only depends on the predicted choice probability. Since $\lambda(P_s)$ is a nonlinear function of *z*, the equation is identified even if all variables in *z* also appear in *x*. However, identification through functional form requires sufficient variation in *z*[8], and correct specification of the functional form of both selection and earnings equations. In practice,

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[6]Falaris (1987) applies the Lee correction with a nested logit choice model in a study of migration between states in Venezuela.

[7] While joint normality is convenient for calculating the expressions below, it is sufficient that the joint distribution $F(u, \eta)$ has $E(u|\eta)$ linear in η.

reliable identification requires an exclusion restriction, i.e., finding a variable that affects only the choice probability but not the outcome.

Estimating the selectivity corrected earnings equations (10) is straightforward but interpretation of the coefficient estimates can cause confusion. A "positive selection bias" (Lee 1995a) occurs when E(u_s|x,s chosen)>0, i.e. the individuals who chose the alternative s, earn more than a randomly chosen individual would earn at this schooling level (controlling for the observed differences in x). Since the expression (10) involves a density and a distribution function, both of which are always positive, positive selectivity requires that $\sigma_{u_s,\eta_s^*} < 0$. On the other hand $\frac{-\phi(.)}{F_s} = E\left(\eta_s^* | \eta_s^* < J(\gamma_s z)\right)$ is an expectation of a right truncated normally distributed variable (Maddala 1983). This expression is an increasing function of the choice probability and an increasing function of the index $\gamma_s z$.[9] Therefore, positive selection implies that those with *low* predicted probability of choosing s but who, nevertheless, chose the alternative s, have a *higher* expected value of u_s than those with a *high* predicted choice probability. This counterintuitive result can be understood by examining the selection rule in equation (9). A low predicted choice probability, i.e. a low value of the index $\gamma_s z$, implies that in order for the equation (9) to be satisfied, η_s has to take a very small value. According to (8) a small η_s is related to a big ε_s. Thus, a person that chooses s, even if by observed characteristics the choice appears unlikely, probably receives exceptionally high utility from that choice and this could well be related to high expected earnings.

Finally, a word of warning should be attached to all interpretations. A wrong exclusion restriction will have serious consequences for the estimates. Suppose that a variable has a positive effect on both choice probability and earnings conditional on the choice, but is excluded from the outcome equation. Then since $\lambda(.)$ is an increasing function of this variable, a coefficient estimate for σ_{u_s,η_s^*} tends to be positive implying *spurious negative* selection. On the other hand, dropping this variable from the choice equation would result in

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[8] $\lambda(.)$ is roughly linear over the body of permissible values if the index $\gamma_s z$, but becomes nonlinear at extreme values of the index. Identification through functional form exploits this tail behavior (Vella 1998).

[9] $$\frac{\partial}{\partial P}\left(\lambda(P)\right) = \frac{\Phi^{-1}(P)}{P} + \frac{\phi(\Phi^{-1}(P))}{P^2} > 0, \quad for\ P \in (0,1).$$

true positive selection (an unobservable that affects positively the choice probability and the outcome). Therefore, a careful choice of identifying exclusion restrictions is crucial for the validity of the results. Unfortunately, the theoretical grounds on the choice of variables to be excluded are often weak. For example, in the basic Roy model (Roy 1951), individuals choose their occupation based on expected earnings in different occupations, suggesting that there are no variables that would affect the choice without affecting the earnings in these occupations.

4.2.3 Calculating the opportunity costs

The expected opportunity costs for individual *i* who chose the schooling level *k* instead of level *s* are given by

$$E\left(y_{is} | k \neq s\right) = x_i \beta_s + E\left(u_{is} | k \neq s\right). \tag{11}$$

The first term in this expression is simply a product of the individual characteristics and the skill prices at the schooling level *s*. The second term is more complicated since it involves conditional expectations of random variables that cannot be observed. However, Lee(1995a) shows that, given the parametric forms for P_k, P_s and $E(u_s$|s chosen); $E(u_s$|k chosen) can be evaluated. Lee's lengthy derivation will not be repeated here, but the resulting expression for the opportunity costs in the case of logit choice probabilities is of a rather simple form.

$$\begin{aligned} E\left(u_s | k\ chosen\right) &= \sigma_{u_s,\eta_s^*} \frac{1}{1-\frac{e^{z\gamma_s}}{\sum_{j=1}^{M} e^{z\gamma_j}}} \cdot \phi\left(\Phi^{-1}\left(\frac{e^{z\gamma_s}}{\sum_{j=1}^{M} e^{z\gamma_j}}\right)\right), \\ &= \sigma_{u_s,\eta_s^*} \cdot \frac{\phi\left(\Phi^{-1}\left(P_s\right)\right)}{1-P_s} = -\sigma_{u_s,\eta_s^*} \cdot \frac{P_s}{1-P_s} \lambda\left(P_s\right) \end{aligned} \tag{12}$$

which depends only on the probability of choice *s* . Therefore, the expected value of the error term in invariant with *k*. This invariance property follows from the sample selection correction method. The choice of schooling level *s* only depends on whether it maximizes utility.[10] If s was not chosen, there is no additional information in knowing what the actual

[10] Note that the choice probability is the probability that a random variable $\eta_s^* < J\left(\gamma_s z\right)$. The selectivity correction term depends on the expectation of this truncated normal variate. In the case

choice was. This is clearly a restriction in the potential covariance patterns between J error terms in the utility functions and the error term in the outcome equation, basically requiring that all covariance terms have the same sign (Schmertmann, 1994), but at the same time it allows a simple way of calculating the expected values in errors in the alternatives that were not chosen.

Another interesting implication of (12) is that when there is positive selectivity on unobservables, $E(u_s|x, s \text{ chosen}) > 0$, the expected opportunity cost of individuals who did not choose this alternative will be less than population average, $E(u_s|x, k \text{ chosen}) < 0$ (Lee 1995a). The result is similar as in binary choice models (Heckman 1990).

4.3 Data

The data set used in this study is created by merging test score data from the Finnish Army with census data files. The data include all the men who were serving in the army in 1982.[11] Since military service is compulsory in Finland this covers almost the entire male cohort. Usually men enter the army at the age of twenty, but it is possible to apply earlier as a volunteer or postpone the service up to the age of 30. The typical high school graduation age in Finland is 19, so most high school graduates serve in the military before pursuing further studies at universities or other higher education institutions. Since this study focuses on post high school schooling decisions, only men with a recent high school diploma and no further education at the time of entering the army are included in the sample.

The army gives all recruits a general ability test that is used as an ability measure in this study. The test consists of two parts. The first part measures basic cognitive skills: mathematical ability, verbal ability and logical reasoning. This device has been in use since the 1950's. In 1982 the army introduced a personality test to measure various psychological factors. This personality test includes a section labeled as leadership inventory. This section measures leadership motivation, energy, achievement motivation, self-confidence, considerateness, sociability, sense of responsibility and masculinity. A brief description of

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that the choice s was not made, the only available information is that $\eta_s^* > J(\gamma_s z)$. So calculating expected values simply involves left truncated instead of a right truncated normal variates.

[11] The army records were not readily available prior to 1982. Since 1982 all records are stored in a database using social security numbers that make merging with other data sources possible.

test items is in the appendix. The test scores are used in selection of men into officer's training. Since the officer training extends the length of service from the minimum of eight months to eleven months,[12] there may be an incentive to mispresent ones ability. However, the test incorporates some consistency checks and according to the army psychologist the correlation patterns across the different test items do not suggest that such behavior is common among test takers.[13] In this study, the test score variables are simply used as proxies for individual characteristics.

The labor market data are based on the longitudinal census data files of Statistics Finland. The data cover the entire population living in Finland in any of the census years 1970, -75, -80, -85 and -90. For the 1990's the same information is available on the annual basis from the Labor Force Statistics of Statistics Finland. In this study, the labor market outcomes are recorded in 1994, twelve years after army, when the median age of the men in the sample is 32. These data include register-based information of the highest level of schooling completed and taxable earnings. Here, annual taxable earnings are converted to monthly earnings using the information on months in employment. This information on the labor force status is based on the number of months that the employer paid social security contributions for the employee. There is some measurement error in this variable leading to extremely low and extremely high calculated monthly earnings for some individuals. To minimize the impact of measurement error, only the men who earned at least FM 4000 per month and who were employed for at least 6 months, were included in the wage regressions. Income from self-employment is not included and the men whose main income source was other than wages and salaries are also dropped. These exclusions apply only to wage regressions; there is no reason to exclude these individuals from the schooling choice equations.

Another source of uncertainty is related to schooling measures. The data originate from a register of completed degrees and contain no information on spells of education that did not lead to a degree. In particular, some who are recorded as having high school education only, actually have an unfinished university degree. Here the problem is partially taken into account by dropping the men whose highest degree in 1994 was high school diploma but who were

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[12] This refers to the system in 1982. After a recent reform, the possible lengths of service are 6, 9 or 12 months.

[13] This information is based on personal communication with the director of Defense Forces Education Development Center, Juhani Sinivuo.

reported to be students in the 1985 or the 1990 census or who were still enrolled in university in 1994.

To capture the essential elements of the schooling alternatives, post-high school education is categorized into three different groups based on the educational classification of Statistics Finland. The first group is composed of those with upper secondary level vocational education. A typical education in this group is lower business degree "merkonomi" from a commercial college. Here the group is labeled "vocational" for simplicity. The second group has lower tertiary education. Statistics Finland classifies this as "lowest level of higher education". In practice, most of these men are engineers and, hence, labeled as "technical" in this study. A rather small group of men with a lower candidate level education is also included in this group. Finally, the third group consists of those with at least upper candidate level (Master's degree) university education.[14] Naturally, the fourth option is not to continue schooling beyond high school. These men are classified here as "high school". The alternatives can be ranked based on average length of education required, but it is not evident how well such single dimensional ranking describes the choice mechanism.

In addition to labor market outcomes the census files were also used to gather data on parents. The data contain information on both parents' education, occupation, and earnings measured in 1980, approximately at the time when their sons were making their post high school investment decisions. If no information on one or both parents were available, it is assumed that the parent was not present in 1980 and an indicator variable for missing variable is created.

Table 1 contains descriptive statistics of the sample broken down by the level of schooling. There is no natural scale for the test scores. In order to make comparisons between the magnitude of their coefficients easier, these variables are normalized to have a zero mean and unit variance. Also, there is no real labor market experience measure in these data. Here (potential) work experience is constructed based on time elapsed since the graduation date.

[14] Corresponding education levels in Statistics Finland education classification are 3 and 4 for vocational, 5 and 6 for technical, and 7 and 8 for university education. This classification is, of course, arbitrary but seems to capture some of the essential differences between groups. For example, those with "lower candidate level education" (level 6) are in all observable characteristics closer to "lowest higher education" (level 5) than "upper candidate level" (level 7).

This underestimates the work experience, especially for the university graduates, who often acquire significant amounts of experience before graduation.

Table 1. Means of variables by the level of schooling

	High school	Vocational	Technical	University
Test scores				
Verbal ability	-0.15	-0.17	-0.06	0.31
Mathematical ability	-0.24	-0.28	0.01	0.43
Logical reasoning	-0.15	-0.21	0.06	0.26
Leadership motivation	-0.10	-0.10	-0.09	0.23
Energy	-0.15	-0.05	0.03	0.12
Achievement motivation	-0.21	-0.15	-0.04	0.31
Self confidence	-0.14	-0.13	-0.04	0.25
Prudence	-0.30	-0.12	0.07	0.23
Sociability	-0.03	-0.027	-0.07	0.11
Sense of responsibility	-0.25	-0.11	-0.02	0.24
Masculinity	-0.08	0.05	0.11	-0.08
Family backgroud				
Mother’s ed basic	0.58	0.59	0.55	0.42
Mother’s ed secondary	0.29	0.29	0.32	0.33
Mother’s ed higher	0.09	0.09	0.10	0.22
Mother’s annual earnings in 1980	33,200	31,500	31,000	36,700
Father’s ed basic	0.44	0.49	0.46	0.34
Father’s ed secondary	0.31	0.28	0.30	0.29
Father’s ed higher	0.14	0.13	0.16	0.31
Father’s annual earnings in 1980	65,900	60,000	60,400	76,100
Labor market variables				
Wage earner*	0.69	0.75	0.83	0.61
Work experience	12.1	7.9	5.9	4.6
Monthly earnings	11,600	11,400	12,400	15,500
N	893	2411	1604	1954

*Months in employment ≥ 6, wage income > other income, and monthly wage > 4000 mk. Only wage earners are included in the calculations of average monthly earnings.

Comparing the means of the variables across different levels of schooling reveals that schooling choice is related both to the measurable personal characteristics and family background. The university graduates have clearly the highest test scores in cognitive tests but also in personality tests. Those who chose "technical" schooling rank second in most tests. There is not much difference in the cognitive skills between those who ended their schooling after high school and those who continued in vocational schools. The same observation can be made looking at the family background variables. The proportion of men whose parents had higher education increases and the proportion of men whose parents had only compulsory schooling decreases as own schooling level increases. Again there is not much difference in family background between the high school and vocational school groups.

4.4 Empirical results

4.4.1 Correlation structure in the test scores

With a large number of test scores it is reasonable to ask whether they really measure different characteristics or whether the information could be summarized in some simple indices. To shed some light on the issue, Table 2a displays the pairwise correlation coefficients of the test score variables. The first observation from the table concerns the cognitive skill measures. These measures have pairwise correlation coefficients around 0.5 but the correlations between the cognitive skill measures and the other test scores are not very high. Among the other measures there are some very high correlations. For example, the pairwise correlations between sociability, leadership motivation and self-confidence are around 0.7. Unfortunately, there is no information available on the reliability of any of the measures. Therefore, it impossible to say whether the difference in these scores is just measurement error.

Table 2b presents results of principal component analysis of the test score variables. The first principal component is simply the linear combination of the individual scores that has the maximum variance. In the psychometrics literature this measure is often called general ability or g. Since here the test scores also include non-cognitive components, this terminology may be somewhat misleading but is adopted for simplicity. In fact, the first principal component appears to be more heavily loaded with the personality test scores than the cognitive scores. Most interestingly, the second component clearly distinguishes the cognitive and non-cognitive tests. Also the third and fourth principal components have rather clear interpretations. The third loads positively on achievement "motivation", "prudence" and "sense of responsibility", all traits that could have something to do with future orienteness or, using more common economics terminology, the rate of time preference. The fourth component appears to load positively almost entirely on the "masculinity"[15] measure. Together these four components explain three quarters of the variance in the test scores.

Table 2a Pairwise correlations between the test scores

	Ver.	Mat.	Log.	Lea.	Ene.	Ach.	Sel.	Pru.	Soc.	Res.	Mas.
Verbal	1.00										
Math	.51	1.00									
Logic	.46	.50	1.00								
Leadership	.19	.12	.16	1.00							
Energy	.17	.12	.14	.69	1.00						
Achievement	.21	.19	.18	.62	.61	1.00					
Self confidence	.24	.17	.19	.69	.61	.46	1.00				
Prudence	.09	.07	.08	.24	.33	.35	.32	1.00			
Sociability	.15	.07	.11	.77	.60	.43	.70	.10	1.00		
Responsibility	.18	.11	.14	.52	.47	.49	.49	.59	.40	1.00	
Masculinity	.07	.12	.06	.14	.31	.18	.22	.12	.13	.04	1.00

Table 2b Principal components

Eigenvectors	1st principal component	2nd principal component	3rd principal component	4th principal component
Verbal	0.17	0.53	-0.00	-0.08
Math	0.14	0.58	0.00	0.06
Logic	0.15	0.55	-0.01	-0.08
Leadership	0.42	-0.13	-0.20	-0.15
Energy	0.39	-0.13	-0.10	0.16
Achievement	0.36	-0.04	0.10	0.03
Self confidence	0.39	-0.07	-0.16	-0.05
Prudence	0.23	-0.08	0.72	0.14
Sociability	0.36	-0.16	-0.40	-0.22
Responsibility	0.34	-0.10	0.46	-0.15
Masculinity	0.13	0.01	-0.16	0.92
Eigenvalue	4.23	1.80	1.17	1.01
Proportion of variance	0.38	0.16	0.11	0.09
Cumulative proportion	0.38	0.55	0.65	0.75

[15] The fourth principal component turned out to have no significant effects in neither schooling choice nor earnings, and were subsequently dropped below.

4.4.2 Simple wage equations

As a first indication of the importance of the characteristics measured by the test scores simple ordinary least squares regression models were estimated. Log monthly earnings is explained by measures of schooling, family background and the test scores. The first column in Table 3 includes only education and experience as explanatory variables. According to the estimates the return to the post high school education is about 6% for vocational, 21% for technical and 47% for the university education. The experience profile is concave with the return to the first year of experience about 6%. The explanatory power of the equation is rather weak compared to typical cross-section earnings functions that usually explain about 30% of the variation in earnings. This is mostly due to a restriction in range of the explanatory variables. All the men in the sample have at least high school education and are in their early thirties. Therefore, if the residual variation in the sample is of same magnitude as in a typical cross-section regression, the lower explained variation leads to a low R^2 measure.

In the next column, the test score variables are added to the equation. This reduces slightly the coefficients of the schooling variables, suggesting a small upward omitted variable bias in the estimates of the first column. Of the individual test items the mathematics score, "leadership motivation" and "sociability" appear to have positive effects on earnings, and interestingly, "sense of responsibility" a negative effect. Overall, the test scores do not add much to the explanatory power of the equation. Apparently, even observing a large number of individual characteristics, that would usually be picked up by the error term, does not explain the earnings differences within a group of men with similar education and experience.

Adding eleven test scores into a regression equation is not necessarily a very efficient procedure. Measurement errors in the test scores will cause biases in the coefficients and collinearity between different scores will inflate the standard errors of the estimates. In column three, only the three first principal components are used. This simply imposes a constraint in the model that the test scores can only enter in the equation as linear combinations given by the first three eigenvectors. Since the principal components are by construction orthogonal, there is collinearity between them. Also the measurement error problem is reduced by "averaging" over the different test scores.

Table 3 First wage equations

	(1)	(2)	(3)	(4)	(5)
Vocational	0,062*	0,065*	0,067*	0,065*	0,064*
	(0,028)	(0,028)	(0,028)	(0,028)	(0,028)
Technical	0,212*	0,201*	0,208*	0,210*	0,197*
	(0,033)	(0,033)	(0,033)	(0,033)	(0,033)
University	0,474*	0,436*	0,449*	0,453*	0,418*
	(0,034)	(0,035)	(0,035)	(0,035)	(0,035)
Experiece	0,060*	0,058*	0,059*	0,059*	0,057*
	(0,008)	(0,008)	(0,008)	(0,008)	(0,008)
Exp2(x100)	-0,222*	-0,212*	-0,212*	-0,218*	-0,212*
	(0,066)	(0,066)	(0,066)	(0,065)	(0,065)
Verbal		-0,007			-0,006
		(0,006)			(0,006)
Math		0,026*			0,027*
		(0,006)			(0,006)
Logic		0,008			0,004
		(0,006)			(0,006)
Leadership motivation		0,029*			0,024*
		(0,009)			(0,009)
Energy		0,001			0,002
		(0,007)			(0,007)
Achievement		0,010			0,011
motivation		(0,006)			(0,006)
Self confidence		0,013			0,012
		(0,008)			(0,008)
Prudence		0,002			0,005
		(0,006)			(0,006)
Sociability		0,017*			0,017*
		(0,008)			(0,008)
Sense of responsibility		-0,016*			-0,015*
		(0,007)			(0,007)
Masculinity		0,006			0,006
		(0,005)			(0,005)
1st principal			0.027*		
component			(0.002)		
2nd principal			0.009*		
component			(0.004)		
3rd principal			-0.021*		
component			(0,004)		
Mother's education				0,011	0,010
secondary				(0,011)	(0,011)
Mother's education				0,011	0,007
higher				(0,017)	(0,017)
Mother's earnings in				0.625*	0.481*
1980 (x10^6)				(0,231)	(0,231)
Father's education				0,027*	0,020
secondary				(0,011)	(0,011)
Father's education				0,002	-0,009
higher				(0,016)	(0,016)
Father's earnings in				0,841*	0,749*
1980 (x10^6)				(0,122)	(0,122)
N	5075	4950	4950	5075	4950
R^2	0,172	0,202	0.199	0,188	0,213

All equations include a constant; columns 4 and 5 also dummy variable for missing parents. Standard errors in parentheses. Comparison group is high school graduates (in columns 4 and 5 high school graduates whose mother and father have no post-compulsory schooling). Coefficients that are significant at 5% level marked with *.

The first principal component (general ability) has a clear positive impact on earnings. Also the coefficient of the second component (academic skills) is statistically significant and positive. In contrast, the third principal component appears to have a negative sign. This result is probably best interpreted by noting that the third component is negatively loaded on "leadership motivation" and "sociability" measures both of which have a positive effect on earnings.

In columns 4 and 5, measures of parents’ education and earnings were added to the equation. Parents' education was described with dummy variables for secondary and higher education and an indicator for missing information. The comparison group is parents with only compulsory schooling. The results indicate that both mother's and father's earnings have a positive effect of son's earnings but parents' education or even missing parent does not have any effect.

In Table 4, the same regression equations were estimated separately by the level of schooling. Again parents' education did not have any significant effects and the table reports the results from a specification with family background variables omitted. For brevity, only regressions on the first three principal components are reported; below each column the test scores that were statistically significant at the 5% level in a regression of earnings on separate scores are listed. The first notable feature in this exercise were that the test scores explain relatively little of the within education level variation in earnings. The R^2 is greatest 0.13 for university graduates, but just regressing their earnings on a quadratic function of experience results in an R^2 of 0.10. The first principal component is significant and positive in all education levels. The coefficient on the second is only significant on technical schooling group. Also, the coefficients do not vary a lot across schooling levels, suggesting that the skill prices in the different levels of schooling may not be that different after all.

Table 4 Wage regressions by the level of schooling

	High school	Vocational	Technical	University
Experience	-0,344	0,035*	0,031*	0,056*
	(0,630)	(-0,017)	(0,015)	(0,016)
Exp^2(x100)	1,489	-0,107	0,081	-0,045
	(2,555)	(0,125)	0,143	(0,182)
1st principal component	0,023*	0,029*	0,021*	0,030*
	(0,007)	(0,004)	(0,004)	(0,005)
2nd principal component	0,013	0,011	0,014*	-0,001
	(0,011)	(0,006)	(0,007)	(0,001)
3rd principal component	-0,005	-0,018*	-0,033*	-0,024*
	(0,013)	(0,007)	(0,008	(0,008)
N	1456	1739	1198	1430
R^2	0,098	0,050	0,102	0,133
Significant in a separate regression	Logic Leadership	Math	Math Leadership Masculinity	Math Sociability

4.4.3 Schooling choice

Table 5 presents estimates of the OGEV-model for schooling choice. The model is identified by setting the coefficients for the base category (high school) equal to zero. The estimates reported in the table are, therefore, effects of a change in the explanatory variables on the log odds of choosing category s rather than the base category. Since the choice of the base category is arbitrary, a proper test of the effect of a variable on the choice probabilities is a χ^2-test of the hypothesis that all coefficients corresponding to the same variable are zero across equations. This is reported in the last column.

Table 5 OGEV model for schooling choice

	Vocational	Technical	University	χ^2 (3)
Constant	1.20	0.86	0.25	
Verbal	-0.02	-0.11	0.10	7.26*
Math	-0.04	0.21	0.56	77.88**
Logic	-0.02	0.16	0.04	6.86*
Leadership motivation	-0.09	-0.24	-0.03	4.81
Energy	0.09	0.14	-0.18	14.97**
Achievement mot.	-0.02	0.08	0.41	41.50**
Self confidence	-0.11	0.02	0.31	25.77**
Prudence	0.11	0.25	0.35	32.60**
Sociability	0.05	-0.06	-0.22	15.56**
Sense of responsibility	0.10	0.11	0.13	4.29
Masculinity	0.10	0.09	-0.16	23.31**
No mother present	-0.47	-0.36	0.02	4.78
Mother’s education secondary	0.12	0.23	0.30	9.35**
Mother’s education higher	0.29	0.44	0.94	30.40**
Mother’s earnings in 1980	-2.62	-4.50	-0.04	4.33
No father present	-0.32	-0.44	-0.32	5.34
Father’s education secondary	-0.15	-0.03	0.08	5.67
Father’s education higher	-0.01	0.25	0.62	24.47**
Father’s earnings in 1980	-2.27	-2.83	-0.06	6.65*
θ	0.17 (0.52)			
N	6677			
Pseudo R2	0.12			

* significant at 10% level, χ^2 (3) > 6.25;** significant at 5% level, χ^2 (3) > 7.81

The coefficients for university education have the clearest interpretation. A high score in math, achievement motivation, self-confidence and prudence measures increase the odds of completing a university education. The odds of completing a university degree also increase if parents have higher education. The other coefficients are not so clear cut. Some general observations can be made. First, there are not many variables that would help predicting the choice between quitting after high school and continuing in vocational education. Second, the coefficients are not always proportional as would be expected if the choice probabilities were generated from an ordered model. For example, a high score in the verbal test seems to decrease odds of technical education and increase the odds of university education, while a high score in the logic test has an opposite effect. Finally, correlation between the unobservables in the adjacent categories is of reasonable magnitude, but very imprecisely estimated. With the coefficient θ restricted to zero, the OGEV-model reduces to the multinomial logit model. As shown in Small (1994), a t-test of this restriction is equivalent to the Lagrange multiplier - specification test of the multinomial logit model against the OGEV-model as the alternative hypothesis. Since the null hypothesis ($\theta = 0$) is not rejected, the multinomial logit model seems to provide a sufficient description of the choice process.

4.4.4 Selectivity corrected earnings equations

Before estimating the selectivity corrected earnings functions two simplifications were made to the choice model. As the estimates of the earnings functions in the previous section suggested, the information in the test scores can be reasonably summarized in the three first principal components. Also, based on the estimates of the choice model, the covariance between the unobservables can be ignored, which reduces the OGEV-model to the multinomial logit model.

For the exclusion restrictions, the available data provide two possibilities. Firstly, it can be argued that parents' schooling has no effect on earnings. Parents' education can still have an impact on schooling choices either through lowering the costs of schooling or by influencing the preferences of their son. The other possibility would be to interpret some of the personality test scores as measures of tastes for schooling. In particular, "achievement motivation" and "prudence" measures actually include statements of attitudes toward schooling. Since excluding some test scores from earnings equations would make summarizing the rest in the principal components more complicated, I chose to identify the model by excluding parents' education.[16]

Table 6 presents the results from the selectivity-corrected earnings equations. Apparently, the selectivity correction does not alter the results much and, in fact, the selectivity correction terms are insignificant in all four estimated equations. The first principal component (general ability) has a significant and almost equal impact in all equations. The second (academic skills) is significant only in technical education group and the third has a negative impact in all but the high school equation. Parents' earnings also appear important, father's earnings have a positive and statistically significant coefficient in all equations.

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[16] In a specification were all the test scores entered the earnings equations separately (not shown), the results were not sensitive for the choice of the exclusion restrictions.

Table 6 Selectivity corrected earnings equations

	High school	Vocational	Technical	University
Exp	-0,300 (0,629)	0,035* (-0,016)	0,030* (0,015)	0,052* (0,016)
Exp2(x100)	1,302 (2,555)	-0,123 (0,124)	0,075 (0,143)	0,004 (0,182)
1st principal component	0,020* (0,010)	0,021* (0,006)	0,020* (0,004)	0,024* (0,008)
2nd principal component	0,013 (0,013)	-0,001 (0,012)	0,016* (0,007)	-0,008 (0,011)
3rd principal component	-0,003 (0,020)	-0,019* (0,009)	-0,024* (0,009)	-0,025* (0,010)
Father's earnings in 1980 ($x10^6$)	0,915* (0,326)	0,564* (0,226)	0,744* (0,218)	0,447* (0,190)
Mother's earnings in 1980 ($x10^6$)	-0,170 (0,707)	1,230* (0,400)	-0,523 (0,489)	0,321 (0,351)
$-\phi/F$	-0,015 (0,144)	-0,120 (0,100)	-0,169 (0,120)	0,044 (0,091)
N	583	1739	1198	1430
R^2	0,038	0,063	0,116	0,144

Standard errors that are corrected for the presence of generated regressors in parentheses

4.4.5 Counterfactual outcomes

The estimates in Table 6 can be used for calculating the expected earnings in each alternative for an individual with given characteristics. The method involves simply treating the estimated coefficients as sector specific skill prices and plugging in the observed individual characteristics. Information on the actual choices and the estimated choice probabilities can then be used for calculating the expected values of the error terms as described in equation (12). A slight additional complication arises from variables, such as experience, that are directly related to the schooling choices. In the simulations that follow all individuals are assigned 32 years of age (median age in the sample). Then for predicting earnings at the schooling level *s*, all individuals are assigned the level of experience equal to average experience of those 32-year-olds who actually chose the schooling level *s*[17].

[17] The simulation procedure is essentially similar to that in Gregory and Vella (1996). However, their ordered probit choice model is not as attractive for simulations, because it requires an assumption that

The simulation results are summarized in Table 7. In each row average predicted log earnings in all four alternatives are calculated for individuals with same actual level of schooling. The numbers in brackets are standard deviations of the predicted values in the relevant group.

Table 7 Comparison of counterfactual outcomes

	Predicted earnings			
Observed choice	High school	Vocational	Technical	University
High school	**9.29**	9.09	9.08	9.63
	(0.07)	(0.08)	(0.07)	(0.07)
Vocational	9.26	**9.28**	9.07	9.63
	(0.06)	(0.08)	(0.07)	(0.07)
Technical	9.27	9.10	**9.37**	9.64
	(0.06)	(0.07)	(0.07)	(0.07)
University	9.30	9.13	9.11	**9.58**
	(0.07)	(0.08)	(0.07)	(0.07)

The table provides several interesting results. One particularly interesting question is whether the schooling choices are related to comparative advantage. Comparing individuals *i* and *j* who chose schooling levels *k* and *l*, we can state that individual *i* has a comparative advantage in the alternative *k* if $w_{ik} / w_{jl} > w_{jk} / w_{jl}$. Taking expectations of log wages over individuals yields a measure

$$z = E\left(\log w_{ik} + \log w_{jl} - \log w_{jl} + \log w_{ik} | x_i, x_j, s_i = k, s_j = l\right). \qquad (13)$$

If $z > 0$, individuals have on average comparative advantage in their chosen tasks. With four different schooling levels, there are six pairwise comparisons. Five of these cases indicate comparative advantage, the only exception being a comparison between high school and university graduates. High school graduates appear to have a comparative advantage in

the expected value of the schooling equation error stays constant when individuals are moved up on the education level. Specifically, they calculate the expected value of the unobservable factors that influence the latent index variable in the ordered probit, conditional on the index being in a certain interval, and then assume, somewhat awkwardly, that the conditional expectation (that they label under- or overachievement) would be the same in some other interval of the index. On the other hand, Lee (1994) calculates the expected wages in different schooling levels for individuals that have similar observed characteristics. However, this does not reveal what would be an average gain of changing the schooling level for a person with average characteristics of, say, high school graduates compared to an average college graduate.

university education. This is not the only strange result concerning the university graduates. According to the last column in Table 7, the individuals who ended up with a university degree are not the ones with highest expected earnings after a university degree. This anomality is not sensitive to changes in specification: changing the set of the excluded variables or estimating the equations using the individual scores instead of principal components produces qualitatively similar results. There are at least two possible explanations. It may be that the test scores do not measure the relevant skills for university graduates. The tests are designed to discriminate between average recruits, not necessarily between the top percentiles that end up with a university education. Also entrance to universities is restricted, the number of applicants being much greater than the number of accepted students. This may break down the utility maximization assumption on which the estimates of the selectivity corrected earnings equations and the counterfactual outcomes are based on.

Another related observation is that Table 7 can be used for calculating returns to schooling for particular groups. For example, the return to vocational education for those who did attend vocational education (average effect of treatment on the treated) is the difference between predicted earnings of individuals with vocational education minus their expected earnings had they quit after high school. In Table 7, this corresponds to the difference between the second and first column on the second row. Calculating returns to schooling in this manner yields returns of 3% for vocational education, 10% for technical education and 29% for university education. Comparable numbers for potential returns for high school students are -19%, -20% and 34%.

4.5 Conclusion

It is demonstrated that several dimensions of skill are important for schooling choice and earnings determination. The skills cannot be combined into a single dimension so that a one-factor ability model would describe the observed variation. Of course, principal component analysis is just one way of summarizing this data. Another option would involve selecting some measures that could be argued to be relevant for schooling choice and earnings, for example, mathematical and social skills and motivation. However, this choice would be somewhat arbitrary. An attractive feature of the principal component analysis is that no a priori assumptions on what skills might be important were needed.

Another important result is that observed skills explain relatively little of the variation in earnings. Observing a large number of factors that would usually be in the unobservable component increases the predictive power of the equations very little.

The schooling choices appear to be roughly consistent with the comparative advantage story. It seems that students who did not continue in vocational or technical education had a good reason not to do so; they actually did better with just high school education. However, university education seems to be highly rewarded for most people. The evidence on comparative advantage is not very strong. A basic requirement for the existence of comparative advantage is that the rewards to skills are different in the different sectors. The estimates of the impact of the measured skills show a surprising degree of similarity across schooling levels.

On the econometric issues it was demonstrated that the OGEV-model can be used in selectivity correction. However, allowing correlation between the random components of the consecutive alternatives did not change the results. A multinomial logit model appears to be a sufficient approximation of the choice mechanism.

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Appendix. Description of the Finnish Army basic ability test[18]

Part 1, Basic skills (Peruskoe 1)
Each test consists of 40 multiple-choice questions. The raw score is the number of correct answers.

Math: The test consists of simple arithmetic operations, short problems given in a verbal form and completition number series that are arranged according to a certain rule.

Verbal: The questions involve choosing a word that is a synonym or an antonym of a given word, selecting a word pair that displays a similar relationship than a given word pair and to choosing which word does not belong to a given group of words.

Logical: The questions show a matrix of figures with one figure missing. The examinee has to decide which figure completes the matrix.

Part 2, Leadership inventory (Peruskoe 2)
The items are yes/no statements that sum up to the following scores.

Leadership motivation: A person with a high score is socially active and brave. He wants to make plans, and to influence others. He also trusts in his ability to lead others and he takes a responsibility of his actions also in difficult situations. A person with a low score rather remains on the background and avoids situations where he would have to be responsible of planning and leading.

Energy: A person with a high score is vital and action-oriented. He gets started quickly and works effectively and quickly. He has to be almost constantly doing something. A person with a low score is slow and phlegmatic and avoids physical action. He expects that problems solve themselves. Tasks that require speed or constant action are stressful for him.

Achievement motivation: A person with a high score enjoys studying. He sets his goals high. Achievement is important for him and he is confident that he will be successful. A person

[18] Translated and shortened from unpublished Finnish Army documents.

with a low score has a negative attitude towards schooling. He does not have high goals and he puts minimum effort in his work.

Self-confidence: A person with a high score believes in himself. He views himself at least as able and intelligent than others and believes that he will manage in life, if necessary, even without help from others. A person with a low score is uncertain about himself. He gives up easily and blames others for his failures.

Prudence: A person with a high score plans far in the future and considers carefully the results of his actions. He avoids taking unnecessary risks. A person with a low score is impulsive and cannot resist temptations. He gets excited easily but has a careless attitude, and often fails to finish his tasks.

Sociability: A person with a high score seeks company of other people. He enjoys talking and entertaining others. He finds it easy to meet new people and to create new relationships. A person with a low score is quiet and reserved. He has trouble to express himself and he is afraid of public presentations.

Sense of responsibility: A person with a high score has high ethical and moral standards. He is usually trustworthy and takes the responsibility if things go wrong. A person with a low score avoids taking personal responsibility. He has rather loose ethical and moral standards and a tendency to act against commonly accepted norms.

Masculinity: A person with a high score prefers traditionally male dominated occupations. He also prefers action and excitement to emotionally sensitive thinking. A person with a low score is an emotional person. He is intellectually and culturally oriented and prefers female dominated occupations.

Chapter 5

Trends in between- and within-group earnings inequality in Finland

Abstract

This study describes the changes in the wage structure in Finland between 1977 and 1995, and provides a simple explanation based on demand and supply of skills. The study augments the single index model of Card and Lemieux (1996) to incorporate changes in the supply of skills. The augmented model adequately accounts for the changes in the relative wages between groups of different education and experience, but does not capture the changes in within-group distribution.

Keywords: Wage dispersion, return to skill

5.1 Introduction

Studies of trends in the wage distribution typically decompose changes in inequality into changes in earnings differences between some skill groups (for example levels of education) and changes in dispersion within these groups. While changes in between-group inequality can often be reasonably well explained in a simple supply-demand framework, changes in within-group distribution do not fit as well into the standard Economics 1 analysis. In this paper, I present a simple conceptual framework for analyzing both between- and within-group changes in inequality. I also test the hypotheses implied by the model using data from the Finnish Income Distribution Surveys 1977-1995.

The revival of interest in earnings inequality among economists was largely caused by the dramatic increase in the earnings differences in the US during the 1980's (e.g. survey by Levy and Murnane, 1992). The US experience was shared to a lesser extent by other OECD countries, particularly the UK (Gottshalk and Smeeding, 1997). Changes in the wage distribution in Finland have been smaller than in the US or the UK. However, Eriksson and Jäntti (1997) report a substantial increase in inequality between 1985 and 1990. They also find that the change was mostly due to increasing wage dispersion within groups of workers with similar education and experience.

Several explanations for the increase in earnings inequality have been proposed in the literature. Changes in the institutional setting, such as the decline in the real value of the minimum wage and the decline in unionization, may have increased the dispersion of earnings. The greatest increase in inequality occurred in the countries with least centralized wage bargaining, US and UK (Gottschalk and Smeeding 1997). Decentralization may also help explaining the developments in other OECD countries, since even in countries where the unionization rates remained high, there was a movement toward more decentralized wage bargaining (OECD Employment Outlook 1993).

Supply-related reasons, such as changes in the quantity (or quality) of labor supply across skill groups, were advocated, for example, by Katz and Murphy (1992) and, using Swedish data, by Edin and Holmlund (1995). In a world where workers with different educational qualifications are imperfect substitutes in production, an increase in the relative size of a group will decrease the relative wage of that group (and vice versa). However, in most countries the proportion of the workforce with higher levels of education grew simultaneously with the relative earnings of highly educated workers. Therefore, it is clear that changes in relative supplies are not sufficient to explain the observed changes in the relative wages, but some demand side explanations are required as well.

The demand side explanations for widening inequality include the decline in manufacturing employment, maybe partly caused by an increase of international trade. Other explanations relate changes in the relative demand of different types of workers to changes in final product demand, such as increased demand for services. It has been argued that the flow of less educated workers from the highly paid manufacturing jobs to the low productivity service jobs increased wage inequality. However, since wage differences have increased also within industries, even this cannot be a sufficient explanation. An emerging consensus view of the demand side factors emphasizes the role of skill-biased technological change. Advanced technology and skilled labor appear to be complements in production and technological change increases the relative productivity of the more skilled workers. The empirical problem in assessing the merits of this theory is that the rate of technological change and its effect on relative labor demand is hard to measure. Technological change is often the residual factor that is used to explain the changes that cannot be accounted for by the observed factors. Some direct evidence on this issue is provided by the studies that show how, for example, the spread of computing technology has changed the wage structure (Krueger, 1993).

In this paper, I use changes in wage dispersion within narrowly defined age/education groups to identify demand effects. I therefore assume that variability in wages within groups of employees with similar education and experience is partially caused by differences in skills not captured with these human capital proxies. If technological changes cause more skilled workers to become relatively more productive, we would expect an increase in both between-group and within-group wage inequality. However, provided that the within-group skill distribution stays approximately constant, the relative supply changes should only affect between group variation.

The rest of this article is organized following the suggestion by Levy and Murnane (1992). They present three questions that an ideal study on earnings inequality trends should answer, namely:

1. What is the trend in earnings inequality over the period under study?
2. Along what dimensions is inequality changing?
3. What shifts in demand, shifts in supply and/or changes in wage setting institutions are responsible for the observed trend?

I thus begin by documenting the changes in the earnings distribution in Finland during the period from 1977 to 1995 trying to answer the first two questions in section 5.2. The conceptual framework for answering the most difficult third question is formulated in section 5.3. Section 5.4 presents the empirical results and section 5.5 presents some concluding comments.

5.2 Recent trends in the distribution of earnings in Finland

Contrary to popular belief, the wage distribution in Finland is not particularly compressed. Nor are the returns to observed skills, such as schooling, low in international comparison. According to the figures published in the OECD Employment Outlook (1996), the earnings dispersion among full-time full-year workers in Finland in 1991 was roughly in the level with Germany and Italy and clearly higher than in the other Nordic countries. Also, another OECD study shows that in 1992, the earnings difference between men with university education and with only upper secondary education, was the highest among the OECD countries in Finland. For women, this difference was third highest in the OECD (OECD, Education at Glance 1995)[1].

Previous studies on the trends in the earnings inequality in Finland present somewhat mixed results. Using data from manufacturing industries, Asplund (1994) finds only very small changes in the overall wage dispersion between 1980-1992. The wage dispersion declined slightly in the beginning of the 1980's, increased slightly up to the turn of the decade and started a new decline in the early 1990's. However, Asplund finds a large decrease in the return to education between 1980 and 1985 for the white-collar employees. (She had no data on the education of blue-collar employees.) OECD Employment Outlook (1996) that uses data from the Income Distribution Survey, shows a similar pattern. Income dispersion appears to have declined in the end of 1970's and started a slow increase in 1980. Inequality peaked in 1989 and declined slowly after that. The changes have, however, been much more modest than in Canada, US and UK. Results by Eriksson and Jäntti (1997) are quite different. Using data from quinquennial population censuses 1971- 1990, they find a sharp increase in earnings inequality between 1985 and 1990. They note that this increase, which followed a steady decline in inequality, was comparable in magnitude to increases observed in the United States and the United Kingdom. In their decomposition of the changes of earnings dispersion, they find that the 1970's decrease in inequality can be accounted for by changes in observed characteristics and changes in returns to these characteristics, but that the increase in inequality between 1985 and 1990 occurs almost completely in the within-group dispersion.

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[1] It should be noted that due to high tax progression and generous transfer programs, the post-tax income differences in Finland are much smaller. In fact, a recent cross-country study by Atkinsson, Smeeding and Rainwater (1995) shows that Finland had the most equal distribution of household disposable income among the OECD countries in their sutdy.

I start this section by describing trends in the dispersion of earnings based on published data from the Finnish Income Distribution Survey. After that I present more detailed results based on microdata for the years 1977, 1983, 1989 and 1995 of the same survey.

The Finnish Income Distribution Survey produces annual data on the level, formation and distribution of incomes among households. The main income concept is disposable income, but the survey also gathers data on the taxable earnings of individuals who belong to the labor force. It was first conducted in 1977 and since 1983, it has used a rotating panel design where each household is interviewed in two consecutive years and approximately half of the old panel is replaced with a new sample every year. The survey uses stratified sampling and oversamples high income households. The primary sampling unit is the household and all the members of the household are included in the data. The microdata includes weights based on sampling probabilities. These weights are calibrated to match population distribution in income and various household characteristics. The sample size has varied from 46,600 households and 100,821 individuals in 1977 to 9,262 households and 25,229 individuals in 1995. The information in later years is based mostly on administrative registers, interviews are used for some classification information and for some income sources that are not available in registers. Most importantly for the present study, the earnings information is based on tax records that should be highly reliable.

5.2.1 Trends in aggregate time series

Evolution of real earnings

The period from 1977 to 1990 was characterized by strong economic growth. In 1977, the Finnish economy was in recession. The recovery started in 1978 and stable growth continued during the 1980's. An average annual growth rate of the real earnings of the median worker was almost 2.5% between 1977 and 1990. Turnaround in the stable growth came with the economic crisis in 1990. Average real earnings declined for several years and were in 1995 almost at the level of 1990. Figure 1 shows the development of real annual earnings of wage earners by deciles based on the published figures of the Income Distribution Survey. For comparability over time, the figures include all wage earners, therefore combining part-time and full-time workers as well as workers who have been employed only part of the year. Obviously this causes a problem when comparing periods with high and low unemployment rates.

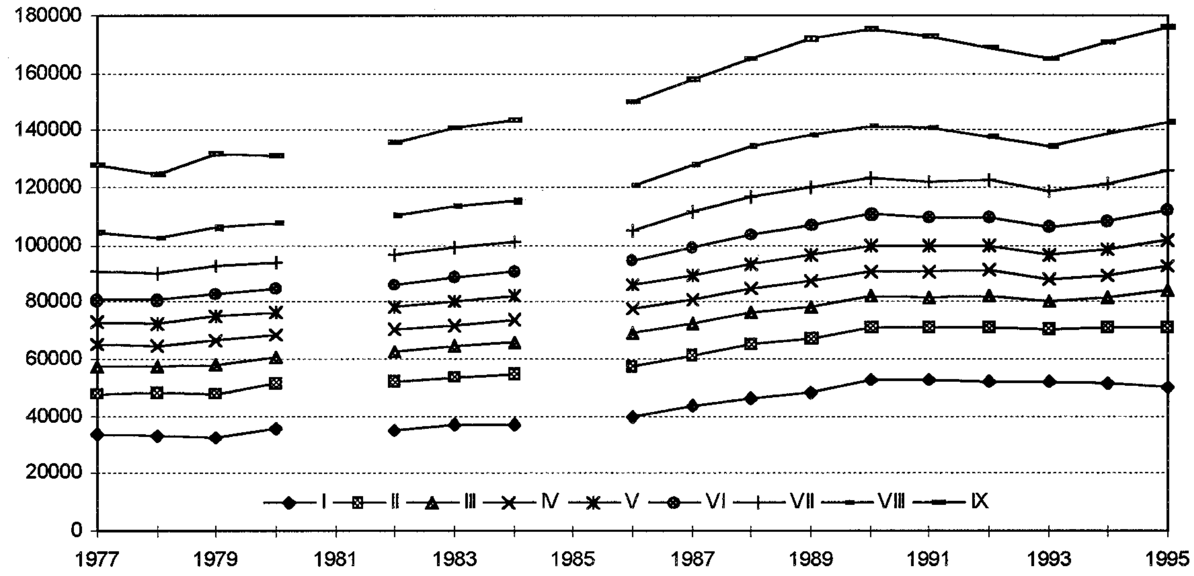

Figure 1. Annual real earnings by decile in 1990 marks.

Note: Data includes all wage earners.

Source: The Income Distribution Survey. The Survey was not conducted in 1981 nor 1985.

Aggregate measures of inequality

Dispersion of earnings among full-time workers has increased for both men and women from 1980 to 1989. Since the gap between men and women simultaneously decreased, the rise in inequality among all employees was less than among men and women separately. Among women the top decile gained compared to the median, whereas among men the widening of earnings distribution occurred both in the 90/50 ratio and in the 50/10 ratio. After 1989 the earnings dispersion declined sharply. Since figures are based on data on full-time year-round workers, this decline was probably partly caused by the relative increase in unemployment among low earners.

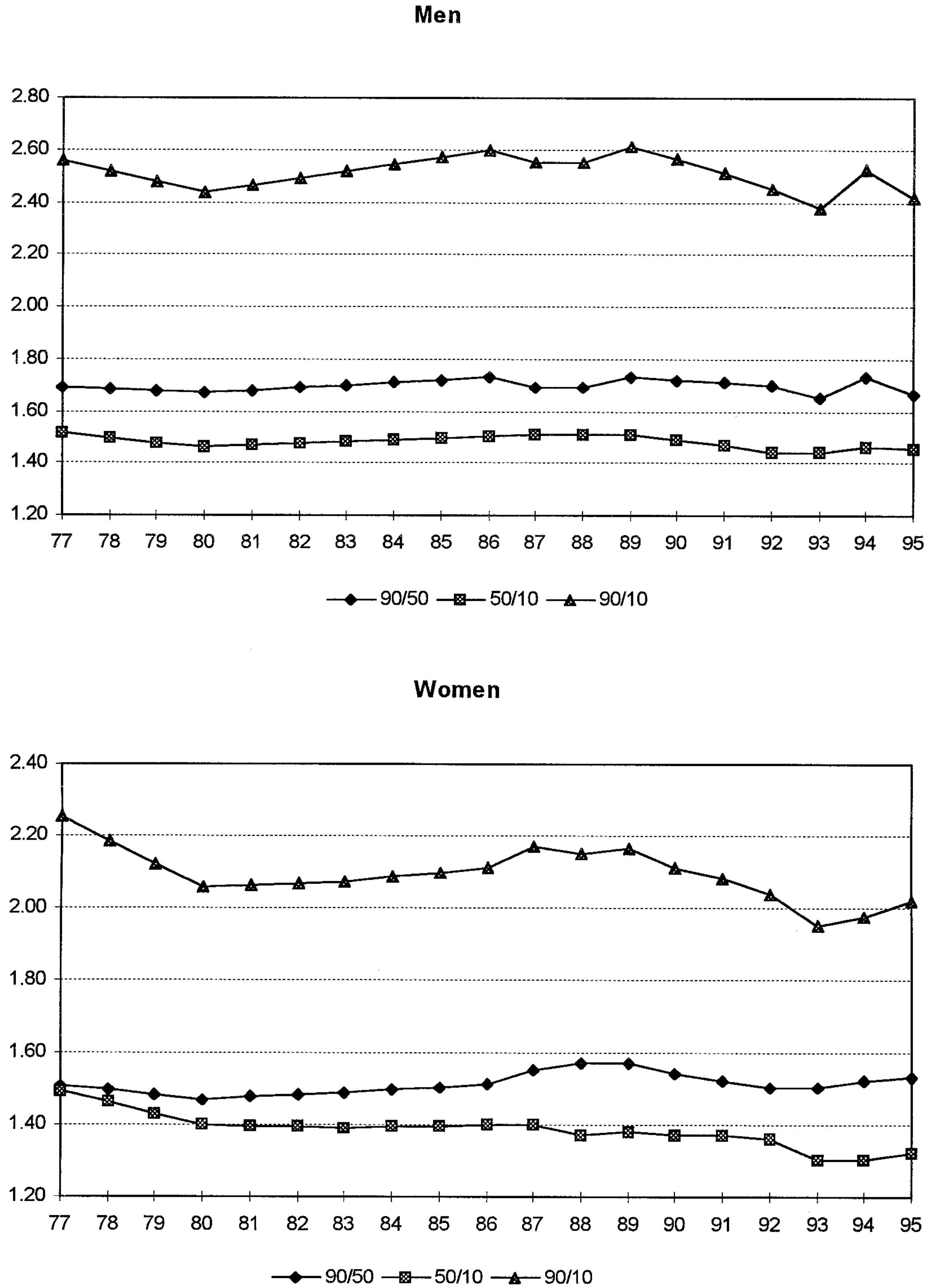

Figure 2. Trends in earnings dispersion.

Note: Data includes only full-time year-round workers. Missing data from the years 1978, -79, -81, -82, -84 and -85 interpolated linearly using the previous and following year with non-missing data. Source: OECD, Employment Outlook 1996 Table 3.1. Year 1977 author's calculations from IDS microdata.

Trends in the return to education

Earnings differences between different educational categories continued their decrease that had started earlier in 1970's (Eriksson and Jäntti, 1997). Some leveling of the trend, and even a partial rebound in returns to education can be seen in between 1985 and 1990 but the decline of relative wages of more educated employees seems to have continued during the 1990's recession. The 1990's development is again problematic: an increase of unemployment was more severe for workers with less education. However, it is not clear whether this increased or decreased the wage differences among the employed workers.

Besides the trends, also the levels of relative earnings are of interest. Due to the classification used by Statistics Finland, a college - high school wage differential, often used in international comparisons, cannot be calculated from these data.[2] Here a more natural comparison is the earnings premium of various education categories compared to employees with only the compulsory nine years of education. On average, during the period under the study, men with university education have earned approximately twice as much as men with only compulsory education. For women the difference has been approximately 80%. Interestingly, lower secondary education (two years of vocational schooling) seems to bring no wage gain. In several years, the average earnings of employees with lower secondary schooling have been less than the average earnings of employees with only compulsory schooling[3].

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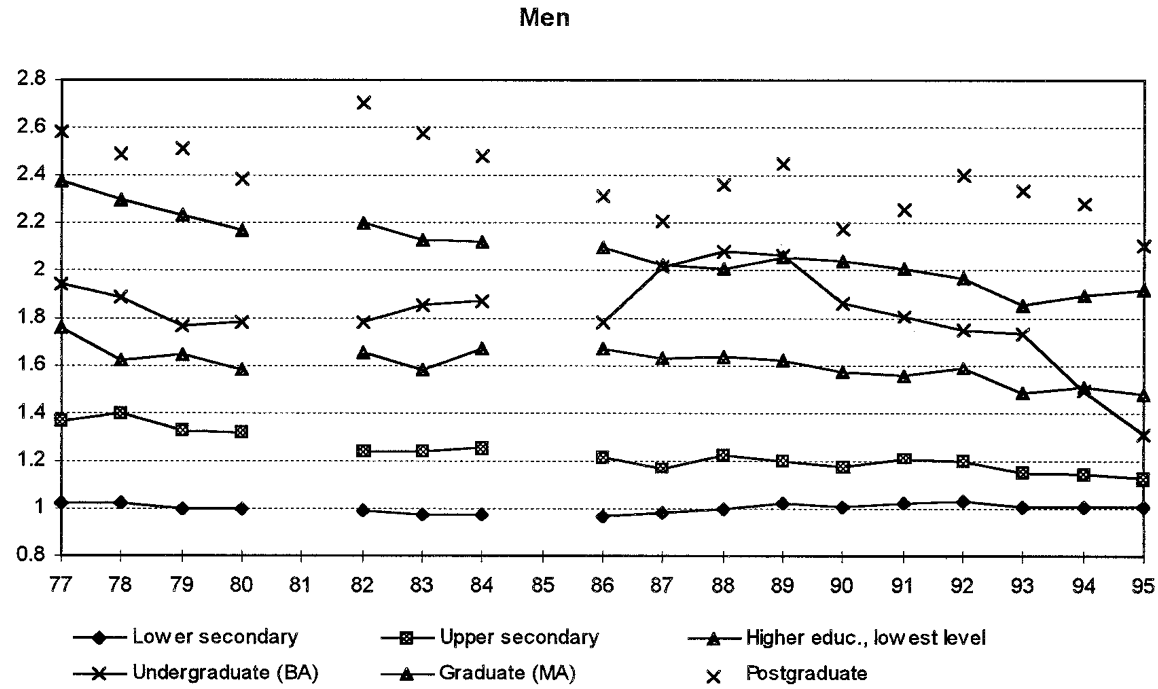
[2] High school graduates are classified as having "upper secondary education". However, in the 1995 census only slightly over 40% of all persons classified into this group had a high school diploma as the highest degree. The rest had completed some vocational upper secondary level education (SVT 1997).

[3] This is partly explained by differences in work experience. The share of workers with only compulsory schooling is higher in the older cohorts.



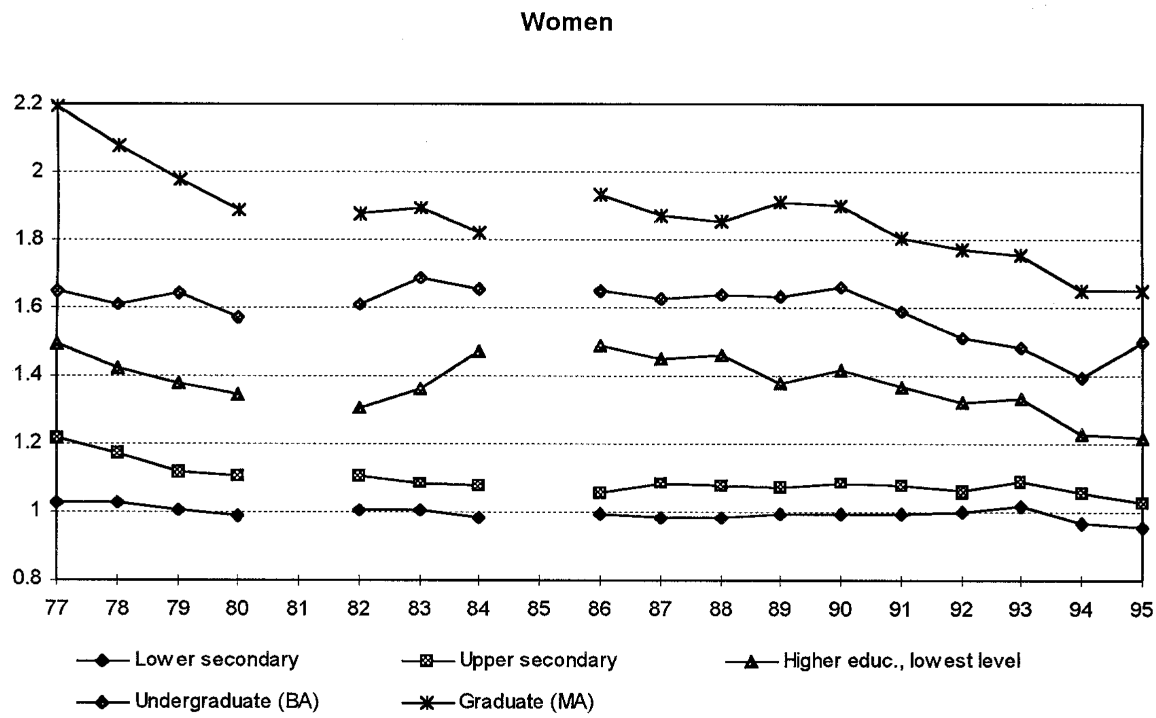


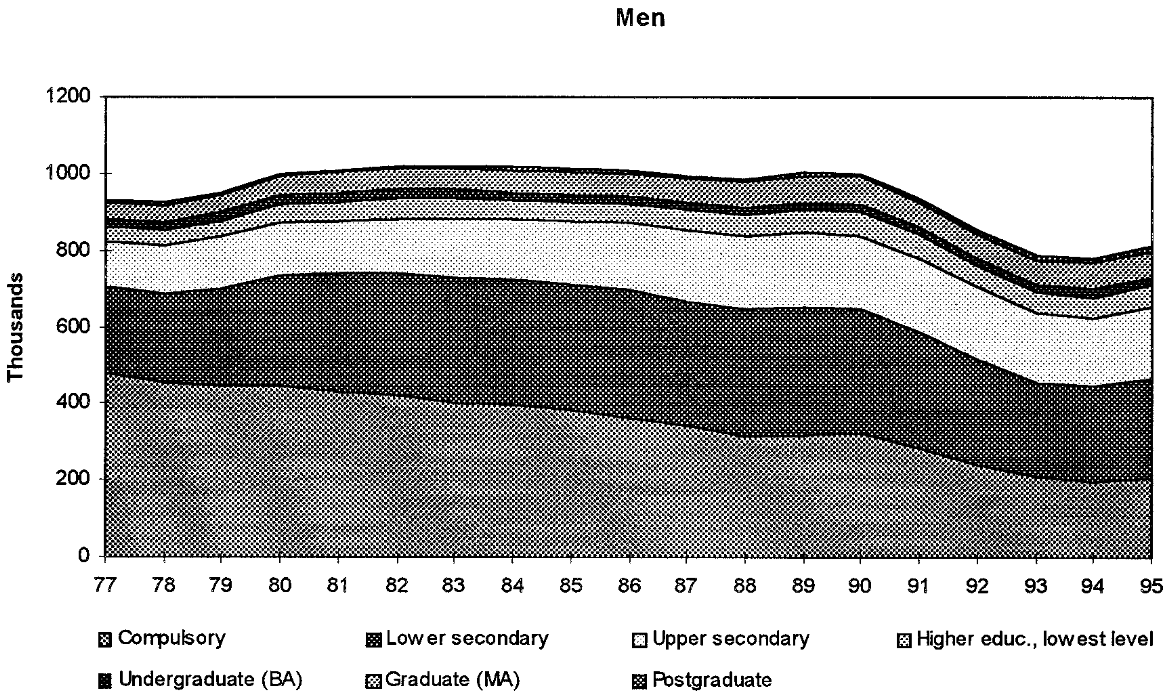
Figure 3. Annual earnings of employees by the level of education compared to earnings of employees with compulsory level education.

Source: The Income Distribution Survey. Data includes both part time and full time workers.

Trends in the supply of skills into labor market

Educational attainment of new labor market entrants has risen quickly. As the new entrants replace the older, less educated workers, the average education level of the labor force increases, though more slowly than the average education level of the new entrants. Therefore, education is rather unequally distributed across the age groups. In 1995, 25-34 year old Finns had a higher average education level than those in the same age group in other Northern-European countries, but 55-64 old Finns had much less education than their Northern-European counterparts. (OECD Education at Glance 1995).

Figures 4a and 4b show the trends in the levels of education among wage earners between 1977 and 1995. Clearly, for both men and women the number of workers with only compulsory education has steadily declined. It appears that the share of lower secondary education first increased but started to decline as the share of those with upper secondary schooling increased. Although the number of employees with higher education has grown more than 50% for men and almost 90% for women, their share of all employees, even at the end of the period, was only about 20% .



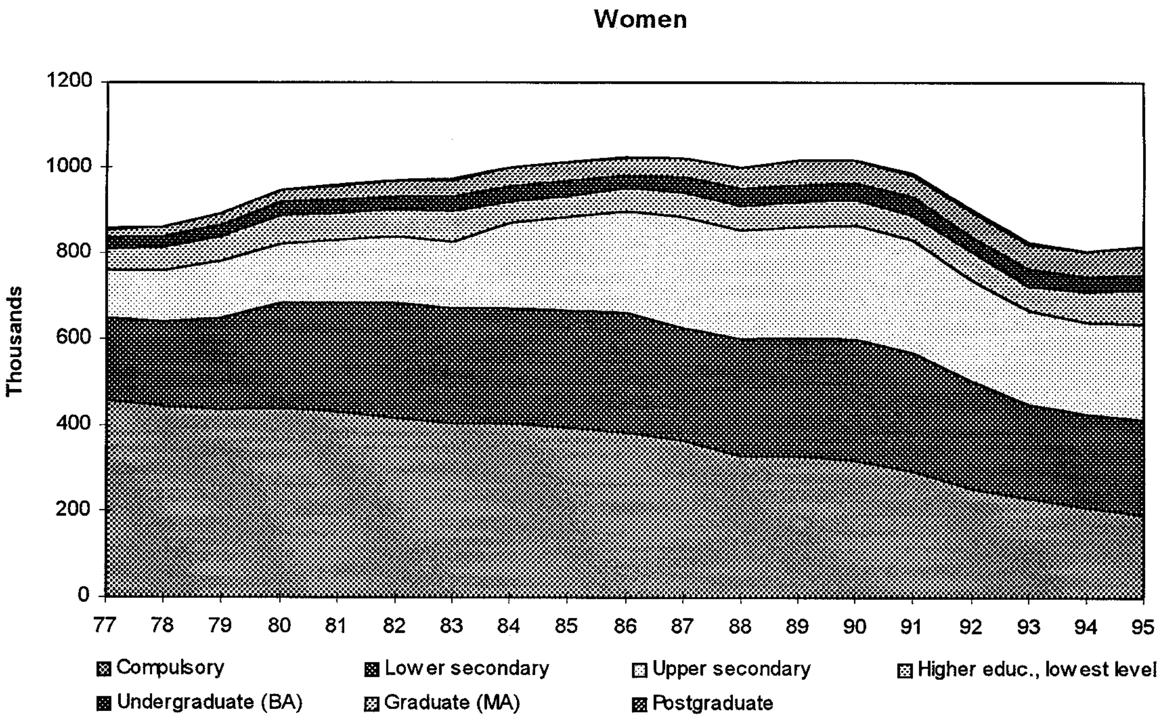


Figure 4. Wage earners by the level of education.

Source: The Income Distribution Survey. Missing data from years 1981 and -85, when the Survey was not conducted, replaced by the average of previous and following years.

Trends in industry composition of the labor force

The composition of employment by industry has also changed rapidly in Finland. In 1970, a quarter of the workforce was still involved in primary production. By 1995, the share of primary production had declined to 8%. Employment in the service sector has increased fairly steadily since 1970 reaching 65% of all workers in 1995. Since the service sector employs women and employees with higher education in larger proportion, the increase of demand for services benefits these groups. As a matter of fact, the service sector has largely absorbed the increase in the labor force, caused by an increase in women's labor force participation rate. Interestingly, manufacturing and construction maintained their share of employment until the economic downturn in the beginning of 1990's. In fact, the absolute levels of employment in both manufacturing and construction were almost identical in 1970 and in 1990. The largest change in the industrial structure occurred in the 1990's when the economic crisis wiped away some 200,000 manufacturing and construction jobs.

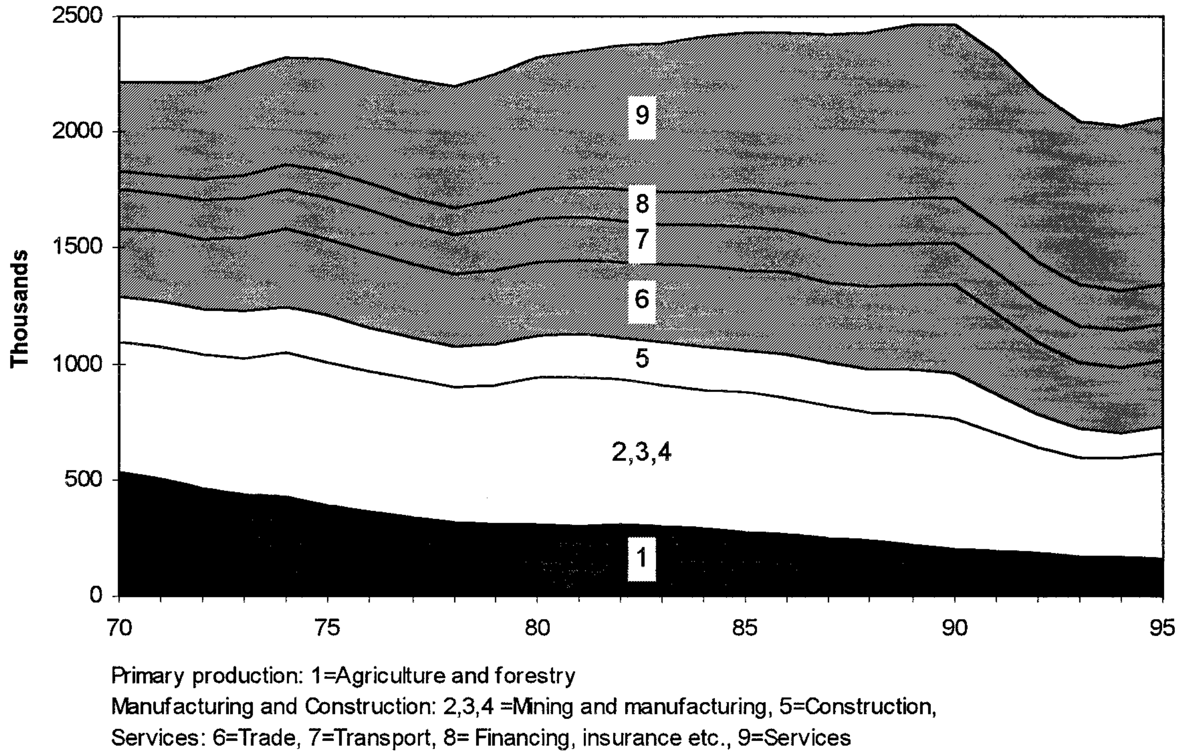


Figure 5 Employment by industry.

Note: Classification based on SIC 1979, after 1990 on SIC 1988. Change in classification corrected by adjusting the figures after 1990 using the ratio of workers in each category according to SIC 1988 and SIC 1978 in the year 1989 when both figures were available.
Source: Labor Force Surveys 1970-1995.

5.2.2 Evidence from microdata

Aggregate figures of the wage distribution mask some important developments in the wage structure. For example, a general increase in women's wages and labor force participation rates has a big impact on aggregate wage distributions. Even more seriously, the changes in unemployment make figures based on annual earnings hard to interpret. Microdata offers an opportunity to control for these changes, at least to some extent, and to focus on earnings measures that are more closely linked to the productivity differences.

In order to analyze the main components of the changes in the wage distribution, I estimated several simple cross-section earnings equations. In each available survey year, log monthly earnings were regressed on a set of dummy variables for six post-compulsory education levels, a quadratic function of age, 12 regional dummies and 9 industry dummies. Estimates are presented in Appendix 1. The estimates show a clear decrease in the return to education between 1977 and 1983. For example, the relative earnings of Master's degree holders compared to those with only compulsory education decreased by about 6 percentage points for both men and women. General compression of education related wage differentials was fairly similar across all education categories. Returns to education increased again between 1983 and 1989, but did not quite reach the 1977 level. During the recession of the 1990's the returns to education decreased. However, employment shares changed drastically during the recession - employment of the less educated fell much more between 1989 and 1995. Therefore, taking into account the employment prospects would result in a more positive impression of the change in the value of education. Interestingly, return to the work experience (proxied by age) behaved quite differently over the period under the study, remaining approximately constant. An inspection of the residual variation reveals that variation in earnings not accounted for by the variables in the model, follows a pattern similar to the that of the return to education. RMSE decreased slightly between 1977 and 1983, increased up to 1989 and decreased again between 1989 and 1995.

In Table 1, I used data from the Income Distribution Survey to calculate various measures of the earnings dispersion. The table shows 90/10, 50/10 and 90/50 decile ratios and the variance of log earnings. Aggregate wage differentials appear to have decreased between 1977 and 1983, though examining men and women separately gives mixed results. Bigger changes occured after 1983. Development from 1983 to 1989 and from 1989 to 1995 has also been more uniform with wage inequality increasing in the former period and decreasing in the latter. Movements in the 90/50 ratio appear to have been slightly bigger, than movements in the 50/10 ratio.

Table 1. Inequality measures

		77	83	89	95
	All				
Variance of logarithms		0.161	0.153	0.163	0.138
Decile ratio	90/10	0.959	0.920	0.967	0.892
	50/10	0.447	0.403	0.419	0.392
	90/50	0.512	0.517	0.548	0.500
	Male				
Variance of logarithms		0.152	0.156	0.173	0.152
Decile ratio	90/10	0.909	0.937	1.012	0.939
	50/10	0.402	0.411	0.446	0.432
	90/50	0.507	0.526	0.566	0.507
	Female				
Variance of logarithms		0.111	0.106	0.118	0.100
Decile ratio	90/10	0.797	0.762	0.821	0.767
	50/10	0.382	0.348	0.360	0.333
	90/50	0.415	0.414	0.461	0.434

Note: Decile ratios are approximated by differences in log wages. For data definitions, see appendix.

In Table 2, I decompose changes in log variance to changes in the observed and the unobserved components. The total change in the variance is simply the difference between variance in log wages in the consecutive surveys, while the unobserved change is the change in the variance of residuals from the regression in Appendix 1. This decomposition shows that the increase in the earnings variance between 1983 and 1989, as well as the decrease between 1989 and 1995, is mainly due to change in the unobserved components.[4] A small increase in dispersion in men's earnings between 1977 and 1983 appears to be accounted for by the regressors but an equally small decrease in dispersion in women's earnings during the same period is not.

[4]Calculations based on the census data in Eriksson and Jäntti (1997) are qualitatively similar but they reported much bigger changes than what is found here. A partial explanation for the difference is that Eriksson and Jäntti used data on annual earnings. Their figures are influenced by the variation in months worked that may account for a large fraction of the variation in annual earnings. However, their main finding of an increase in the within-group inequality in the late 1980's is reproduced here even though the endpoints of the investigated period differ.

Table 2 Change in the variance of log earnings

	83-77	89-83	95-89
All			
Total	-0.008	0.010	-0.025
Observed	-0.005	0.000	-0.011
Unobserved	-0.003	0.010	-0.014
Male			
Total	0.004	0.017	-0.021
Observed	0.006	-0.003	-0.002
Unobserved	-0.002	0.020	-0.019
Female			
Total	-0.005	0.012	-0.018
Observed	-0.001	0.006	-0.004
Unobserved	-0.004	0.006	-0.014

Note: Unobserved change refers to change in the variance of residuals in the regression in Appendix 1 of log mothly earnings on 7 education levels, age, age squared, 9 indusry and 12 region dummies. Observed change is the difference between change in the total variance and the change in the error variance.

To complete this section on recent trends in wage inequality, I calculated the returns to schooling by cohort and survey year. If supply changes play a role in changes in the return to schooling we might expect younger cohorts to have smaller returns to education. The question is, of course, impossible to answer because age, time and cohort effects can not be separately identified, even in a panel setting. Knowing any two will determine the third. Such analysis is still not useless and may detect some patterns in data. In Table 3 one follows a cohort over time by moving horizontally across columns in the same row. Changes within a cohort are due to age or time effects. One follows the same experience group over time by moving upward along a diagonal. Changes along this dimension are due to cohort or time effects. Looking at figures in the same column is equivalent to estimates in a single cross section by age group. For example, the top left corner shows return to schooling for those born between 1949 and 1954 in 1977 when they are 23 to 28 years old. The next figure to the right shows their return to schooling in 1983 when the cohort is between 29 and 34 years of age.

Table 3a. Returns to schooling by cohort and year, men

Birth cohort	1977	1983	1989	1995
1967-72				.0333
1961-66			.0507	.0534
1955-60		.0554	.0723	.0902
1949-54	.0514	.0703	.0874	.0760
1943-48	.0750	.0877	.0971	.0964
1937-42	.0952	.0994	.1082	.0854
1931-36	.1093	.1047	.1143	.0949
1925-30	.1314	.1117	.1137	
1919-24	.1214	.1129		
1913-18	.1180			

Table 3b. Returns to schooling by cohort and year, women

Birth cohort	1977	1983	1989	1995
1967-72				.0472
1961-66			.0639	.0699
1955-60		.0575	.0780	.0790
1949-54	.0594	.0786	.0804	.0807
1943-48	.0774	.0763	.0844	.0800
1937-42	.0865	.0840	.0795	.0728
1931-36	.0936	.0912	.1058	.0753
1925-30	.0903	.0719	.0868	
1919-24	.0853	.0931		
1913-18	.0953			

Note: The figures in graph are coefficients on years of schooling in a cross-section regression by age group. Sampling weights are used in estimation. All equations include 12 region and 9 industry dummies. Data as in Appendix 1, but years of schooling are calculated from the highest degree achieved according to the Statistics Finland education classification.

Two observations are fairly clear. Firstly, youngest workers have the lowest returns to schooling. The top figure in each column is the smallest and this does not appear to be a cohort nor a time effect. Secondly, the returns to schooling appear to be lowest in 1995 no matter whether the cohort or the age is held constant.[5] For the rest of the table it is much harder to detect obvious patterns. Since the figures seem to change more by the rows (cohort constant) and by the columns (year constant) than by the diagonal (age constant), it seems that the age effects are dominating. Of course, the same pattern could be caused by an interaction of the cohort and the time effects.

[5]Standard errors of the estimates vary because the sample size varies by year and age group, but 95% confidence intervals are in most cases around [b-0.01,b+0.01].

In the next section I try to provide an explanation for the observed changes but at this point a short summary of stylized facts might be useful.

During the period from 1977 to 1989, Finland underwent a fairly smooth transition in industrial structure with a shift of workers from primary production to services. At the same time, the share of more educated labor steadily increased. Movements in the wage structure were modest. Returns to the education had a negative trend, though this trend was somewhat reversed in the late eighties. Simultaneously with the rebound of the return to education, unexplained variation in earnings clearly increased. Economic crisis in the 1990's caused quite dramatic changes. Unemployment rates rose for all workers, but the employment losses were much bigger for the less educated. For those who remained at work, real earnings growth ceased and wage dispersion decreased in all dimensions.

5.3 Explanations for the observed changes

A coherent explanation of the changes in the wage distribution should be able to account for changes in both between-group and within-group differences. As noted above, the first task is considerably easier than the second. Understanding changes in the within-group wage dispersion requires some idea of what are the causes of wage differences between workers with similar education and work experience. No doubt, much of this variation is simply random; for some reason wages do not equal productivitities. Part of the observed wage dispersion is just measurement errors in earnings or hours that is always likely to be present in any data. Some fraction of the within-group dispersion could be explained by industry wage differences and increasing the number of covariates in the wage equations would make the within-group dispersion smaller. However, it is questionable whether these factors could explain the changes in the within-group dispersion. But, if within-group wage differences reflect differences in skills that are valued by employers but not observed in the data, changes in the within-group wage dispersion could be explained by changes in demand for those skills (Katz and Murphy 1992, Juhn Murphy and Pierce 1993).

There is not very much evidence on what these unobservable skills that are in growing demand would be. Results obtained by Blackburn and Neumark (1991) and Bishop (1994) suggest that skills measured by standardized tests do not explain a very large fraction of the within-group wage dispersion. Bell (1996) shows that computing skills became more valuable during the 1980's but even in his regressions, the residual variation did not decrease much after adding an indicator of a computer use. Results on the Finnish data are rather similar both in terms of the predictive power of test scores (Uusitalo 1996) and computer use (Asplund 1997)[6].

The best proxy for skills available in commonly used datasets is educational attainment. One might expect that if the within-group dispersion is caused by the unobservable skills, increases in the within-group dispersion would coincide with increases in the return to education. As shown in the previous section, this prediction is roughly consistent with the observed development in Finland. However, if workers with different educational qualifications are imperfect substitutes in production, returns to education are also influenced by changes in relative supplies. Below, I formulate a model that incorporates both changes in the demand for skills and changes in the supply.

5.3.1 Single-skill model

A simple explanation for the observed changes in the between- and within-group inequality was proposed by Card and Lemieux (1996). They formulate a single-index model relating the relative wages to a one dimensional skill index. In their model, a generic increase of the relative productivity of the more highly skilled workers is expected to increase the wage differentials between age and education groups as well as to raise wage inequality within narrowly defined age/education cells. The first part of this section outlines the basic structure of the Card - Lemieux model and restates its empirical implications. The second part extends the model to incorporate supply effects.

First assume a single-index model, where the wage equals the value of the marginal product. This could be the case if the production technology were such that all workers were perfect substitutes. In period t output Y_t is produced using labor inputs from M groups with N_{ht} workers in each group. The relative productivity of workers in group h in period t equals θ_{ht}

$$Y_t = f\left(\sum_{h=1}^{M} \exp(\theta_{ht}) N_{ht}\right) \tag{1}$$

[6] Asplund (1997) also finds that the wage premia for computer use disappeared between 1991 and 1993.

In a competitive labor market the wage equals marginal product. (For simplicity, normalize the price of output to 1, and focus on the real wages)

$$w_{ht} = \frac{\partial Y_t}{\partial N_{ht}} = \exp(\theta_{ht}) f' \tag{2}$$

Log wage of group *h* workers relative to some reference skill group (with θ_{0t}= 0 at all *t*) is then log(w_{ht})=log(w_{0t}) + θ_{ht}. Changes in technology that induce proportional changes in the marginal productivity of all labor inputs (changes in *f'*) leave the relative wages unchanged. However, "skill-biased" technological change changes the productive values of skills, i.e., changes the θ_{ht} . The simplest assumption is that the relative productivity of a group at period *t* is related to the relative productivity at the base period with transformation $\theta_{ht} = g_t\ (\theta_{h0})$ where g_t is a strictly increasing function. Since the same transformation affects all skill groups, a central prediction of the single-index model is that the rank ordering of wages across the skill groups stays constant over time. For example, a linear transformation $\theta_{ht} = \beta_t\ \theta_{h0}$ yields

$$\begin{aligned} \log(w_{ht}) &= \log(w_{0t}) + \beta_t \theta_{h0} \\ &= \log(w_{0t}) + \beta_t \left(\log(w_{h0}) - \log(w_{00})\right) = const_t + \beta_t \log(w_{h0}) \end{aligned} . \tag{3}$$

The wage of a worker is, therefore, a linear function of the base period wage. Allowing for differences in productivity also within skill groups, the single-index model also yields predictions on the percentiles of the wage distribution. Intuitively, a skill-biased technological change, or an increase in relative productivity of the more skilled workers, increases wage differentials between the skill groups, and increases the within-group wage dispersion to the extent that it is driven by differences in unobservable skills.

Equation (3) reveals also another central prediction of the single-index model. The mean wage of a particular skill group in the base period is a sufficient statistic for the mean wage of the group in any other period. Therefore, if there are no measurement errors, and the functional form of g_t (.) is correctly specified, no other variables except the base period wage should have significant effects on the average wages at period *t*.

5.3.2 Application for cell means and quantiles

A single individual is not likely to be observed in several years in repeated cross section data. Even in panel data, an individual does not belong to the same education/experience group in the different years. However, if the within-group skill distribution stays constant, the model yields empirically testable implications in cell means and quantiles.

Card and Lemieux (1996) assume that observed log wage of an individual *i* in the cell *h* at the period *t* consists of the average productivity of the individuals in cell $\overline{\theta}_{ht}$, the individual deviation from the group mean $a_{iht} = \theta_{iht} - \overline{\theta}_{ht}$, which reflects unobserved individual specific productivity factors, and random errors ε_{iht}, which reflect measurement errors, labor market errors etc.

$$\log(w_{iht}) = \overline{\theta}_{ht} + a_{iht} + \varepsilon_{iht} \qquad (4)$$

where a_{iht} and ε_{iht} are normally distributed with zero means variances σ_a^2 and σ_ε^2 The mean log wage of workers in group *h* in the base period is therefore

$$\overline{\log(w_{h0})} = E\left[\overline{\theta}_{h0} + a_{ih0} + \varepsilon_{ih0}\right] = \overline{\theta}_{h0}, \qquad (5)$$

and given that the transformation of the productive value of skills is linear from year to year $\theta_t = \alpha_t + \beta_t \theta_0$ the mean wage in period *t* is

$$\overline{\log(w_{ht})} = E\left[\alpha_t + \beta_t \overline{\theta}_{h0} + \beta_t a_{ih0} + \varepsilon_{ih0}\right] = const_t + \beta_t \overline{\log(w_{h0})}. \qquad (6)$$

Similar expressions can be derived for wage quantiles. Changes in the return to skill also change the within-cell wage distribution if a fraction of within-cell variance in wages is attributed to differences in unobservable skills. The *q*th quantile of wages in the cell *h* is

$$\log(w_{ht}^q) = \overline{\theta}_{ht} + z^q \cdot s(a_{iht} + \varepsilon_{iht}) \qquad (7)$$

where z^q denotes the *q*th percentile of the standard normal distribution and *s*(.) is the standard deviation of within-cell wage distribution. Applying a linear transformation on both observable and unobservable skill components yields an expression that relates wage quantiles on the corresponding quantiles in the base year.

$$\begin{aligned} \log\left(w_{ht}^{q}\right) &= \alpha + \beta_t \overline{\theta}_{h0} + z^q \cdot s\left(\beta_t a_{ih0} + \varepsilon_{iht}\right) \\ &= const_t + \beta_t \log\left(w_{h0}^{q}\right) + s_h \cdot z^q \cdot \delta_h \end{aligned} \tag{8}$$

where

$$\begin{aligned} s_h^2 &= \sigma_a^2 + \sigma_\varepsilon^2 \\ \delta_h &= \left(\beta_t^2\left(1 - R_h\right) + R_h\right)^{1/2} - \beta_t \\ R_h &= \sigma_\varepsilon^2 / \left(\sigma_a^2 + \sigma_\varepsilon^2\right) \end{aligned}$$

The equation (8) can be estimated by regressing wages of the qth percentile of skill groups in the year t on constant, previous period wage and quantile-specific intercepts. If the within-group wage dispersion is driven by changes in the relative demand for skills, the coefficients of the base period wages should be equal across different quantiles. Furthermore, if the return to skill has increased ($\beta > 1$) and if a part of the within-cell variance is measurement error ($R > 0$), the expression for δ_h is negative. Therefore, we would expect the wages of lower quantiles to grow by more and upper quantiles by less than the median.

5.3.3 The effect of supply changes

In order to incorporate changes in the relative supply of different skill groups, a slightly more complicated production technology must be specified. A simple assumption is a CES-production function with M groups and N_{ht} workers in each group, all providing labor input X_{it}. Further, assume that workers are perfect substitutes within same sex-education-experience cell but imperfectly substitutable across groups. This is a special case of the two level CES-production function (Hamermesh 1993, p. 39, eq. 2.41).

$$Y_t = \left[\sum_{h=1}^{M}\left(\sum_{i=1}^{N_{ht}} \exp\left(\theta_{iht}\right) X_{iht}\right)^{\rho}\right]^{1/\rho} \tag{9}$$

Assuming that the relative labor supply is exogenous[7], the wage of an individual i is determined by the marginal productivity at full employment:

$$w_{iht} = \frac{\partial Y_t}{\partial X_{iht}}\Big|_{X_{iht}=1} = \frac{1}{\rho}\left[\sum_{h=1}^{M}\left(\sum_{i=1}^{N_{ht}}\exp(\theta_{iht})\right)^{\rho}\right]^{(1-\rho)/\rho} \times \rho\left(\sum_{i=1}^{N_{ht}}\exp(\theta_{iht})\right)^{\rho-1}\exp(\theta_{iht})$$
$$= \left(\frac{Y_t}{\sum_{i=1}^{N_{ht}}\exp(\theta_{iht})}\right)^{1-\rho}\exp(\theta_{iht}) \quad .$$

(10)

Taking logarithms and using approximation $\sum_{i=1}^{N_h}\exp(\theta_{ih}) = N_h\overline{\theta}_h$ yields

$$\log(w_{iht}) = (1-\rho)\left[\log(Y_t) - \overline{\theta}_{ht} - \log(N_{ht})\right] + \theta_{iht}\,. \tag{11}$$

Mean log wages in cell h at period t are then

$$\overline{\log(w_{ht})} = (1-\rho)\left[\log(Y_t) - \overline{\theta}_{ht} - \log(N_{ht})\right] + \overline{\theta}_{ht}\,, \tag{12}$$

and log wages of qth quantile of cell h

$$\log(w_{ht}^{q}) = (1-\rho)\left[\log(Y_t) - \overline{\theta}_{ht} - \log(N_{ht})\right] + \overline{\theta}_{ht} + z^{q}\cdot s(a_{iht} + \varepsilon_{iht}). \tag{13}$$

$s()$ denotes the standard deviation of the within-cell wage distribution. Let us suppose again that the relative productivity is related to the relative productivity at the base period by a linear transformation $\theta_t = \alpha_t + \beta_t\,\theta_0$. Using the expressions above, cell means and quantiles of wages can then be written as a function of the base period wages and supply changes

$$\overline{\log(w_{ht})} = const + \beta_t\,\overline{\log(w_{h0})} - (1-\rho)\left[\log N_{ht} - \log N_{h0}\right] \tag{14}$$

$$\log(w_{ht}^{q}) = const + \beta_t\,\log(w_{h0}^{q}) - (1-\rho)\left[\log N_{ht} - \beta_t\,\log N_{h0}\right] + s_h\cdot z^{q}\cdot\delta_h\,. \tag{15}$$

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[7]This is not very far from reality in Finland were the supply of educated labour mainly depends on the number of slots in the government-run education system.

Both of the above equations are nonlinear in parameters, but with β close to one, the terms in square brackets can be reasonably approximated with changes in log supplies.

The equations (14) and (15) are the equations to be estimated. Extending the single-skill model to incorporate supply effects simply adds in the equations an additional cell-specific term that reflects changes in the relative supplies. If the productive value of the skills increases (β_t > 1) and relative supplies stay constant, both the between-group wage differences and the within-group wage dispersion increase. However, a change in relative supply only affects between-group differences with no effects on the within-group differences. Note also that the supply change identifies the intergroup elasticity of substitution, σ (Hamermesh 1993):

$$-1/\sigma = -(1-\rho)\,. \tag{16}$$

5.4 Empirical results

To estimate the augmented single-skill model, I first divided the data from each survey into 48 cells based on sex, six education categories[8] and ten-year age-intervals[9]. The self-employed and those who had worked for less than six months in the survey year were excluded from the data. For this restricted sample, a measure of monthly wage was constructed based on the annual taxable wage and salary income and information on the number of months spent at paid employment, counting every month in part-time work as half a month. The individuals whose calculated monthly earnings were less than 3000 marks in the 1995 prices were dropped. This restricted sample, weighted by the sampling weights, was used to calculate means and quantiles of the cell wages.

In order to calculate changes in supply, fever restrictions were applied. Following Katz and Murphy (1992), all members of the labor force were included, no matter whether they were employees, self-employed or unemployed. A weighted sum of months in the labor force over all individuals in the cell was used as a measure of labor supply. Normalizing the labor

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[8]Those with postgraduate education are merged with those with Master's level education.

[9]Groups are 25-34, 35-44, 45-54 and 55-64. The youngest (below 25) and the oldest workers (over 64) are excluded.

supplies in the cells by dividing each by the total supply in each year resulted to a cell employment share, a log of which was included in the estimated equations. Changes in the supply measure are, therefore, driven by the changes in the age structure, the changes in the labor force participation rate and the changes in the education composition of the successive cohorts. On the other hand, changes in the demand conditions only affect this supply measure if the changes in employment opportunities induce changes in participation.

There are some obvious problems in explaining mean cell wages with mean the cell wages in the previous survey and change in employment shares. Both mean log wages and employment shares were estimated from sample data. Both estimates contain sampling errors that may not be negligible since the cohort size is relatively small. The problem is potentially more serious in the employment shares, since they enter into the equation as differences. Errors in the explanatory variables cause bias in the estimates. However, error variances for the estimators can be estimated from the data, and these estimates can then be used to derive consistent estimators using errors in variables techniques (Deaton 1985).

Following this strategy, I first estimated the sampling variances of the estimators using standard formulas for variances of means in complex surveys.[10] The across-cell average of the error variance of means provided an estimate for measurement error in cell means.

$$\hat{V}_t = \frac{1}{M}\sum_{h=1}^{M}\frac{1}{N_{ht}}s_{ht}^2$$

where s_{ht}^2 is the within-cell variance of log wages in cell h A sampling variance of wage quantiles was similarly calculated using a normal approximation.

$$\hat{V}_t = \frac{1}{3M}\sum_{h=1}^{M}\sum_{q}\frac{1}{N_{ht}}k^q s_{ht}^2$$

where $k^q = q(1-q)/\phi(z^q)^2$ and z^q is the qth quantile of the standard normal distribution and $\phi(.)$ the density of the standard normal.

[10]The survey mean estimator is programmed in STATA and takes in the account of stratification, clustering and sampling weights.

To estimate the sampling variance in the change of supply, first the sampling variance of the sum of months in the labor force were calculated. The delta method was then used to derive the sampling error in the change of log supplies.

$$Var(\log N_{ht} - \log N_{h0}) = \frac{Var(N_{ht})}{N_{ht}^2} + \frac{Var(N_{h0})}{N_{h0}^2}$$

Finally, the estimates of the sampling errors and the across-cell variances were used to calculate reliabilities of the explanatory variables and the regressions were run with a standard measurement error correction with known (estimated) reliability ratios (Fuller, 1987). The method is computationally somewhat simpler than the one used by Card and Lemieux (1996), where an estimate of a matrix of measurement errors is deducted from an observed moment matrix. The only difference is that relying on reliabilities ignores covariances between the sampling errors in different variables, which should have little practical importance.

5.4.1 Estimates of the single-skill model

The estimates of the single-index model are presented in Tables 4a and 4b. First in Table 4a cell means of log wages are regressed on the cell means of log wages in the previous survey. Four available surveys result in three panels of regression results. In the columns 1, 4 and 7 of each panel, only previous period wages are used as explanatory variables, producing estimates that are comparable with the estimates in Card and Lemieux (1996). If the demand for skills had increased the coefficients of the previous period wage should exceed one. However, all the estimated coefficients are below unity. In fact, in all but one case out of nine, they are significantly below unity. In columns 2, 5 and 8, relative supply changes are added to the estimated equations. Seven out of nine estimated coefficients of the supply change are negative as expected. The magnitude of the supply effects is fairly stable around -0.05 for the first two panels. These estimates are of borderline significance with t-values around 2. There are no significant supply effects between 1989 and 1995.

If the (augmented) single-index model is an adequate description of wage changes, no other variables except the previous period wages should have statistically significant effects. In columns 3, 6 and 9, mean years of education in the cell, mean age in the cell and sex are added as additional predictors of wages. First, in column 3, where the male and female cells are combined, several of these variables turn out as statistically significant. Between 1977 and 1983, as well as between 1983 and 1989, women's wages grew faster than predicted by the single-skill model. More educated lost ground compared to the model predictions and older workers gained from 1977 to 1983, and lost between 1983 and 1989. The single-index model, even when augmented by supply effects, does not appear to predict wage changes very accurately in the combined data. When estimated separately for men and women, in columns 6 and 9, the augmented single-index model performs better. The only significant effects of the additional variables are in 1983 when more educated men earned less and older men more than predicted, and in 1995 older women earned more than the model prediction. All other coefficients of additional variables are insignificant.

Another test of the model specification is the goodness of fit statistic suggested by Card and Lemieux (1996). If the model is an accurate description of changes in the wage structure, there should be no random component in mean cell wages except for sampling error. Given the estimates of the dependent and indepedent variables $\hat{y} = y + \varepsilon$ and $\hat{x} = x + u$, the model can be written in terms of observable variables

$$\hat{y}_h = \hat{x}_h \beta + \varepsilon_h - u_h \beta \,.$$

Ignoring the covariance terms, the variance of the residual of the above equation $\hat{\eta}_h = \hat{y}_h - \hat{x}_h \beta = \varepsilon_h - u_h \beta - x_h \left(\hat{\beta} - \beta\right)$ can be written as a sum of three terms.

$$Var\left(\eta_h\right) = Var(\varepsilon_h) + \beta Var(u_h)\beta' + x_h Var\left(\hat{\beta} - \beta\right) x'_h \,.$$

An estimator for the covariance matrix of a vector of residuals $\hat{\eta}_h = \left[\hat{\eta}_1, \hat{\eta}_2, \ldots, \hat{\eta}_M\right]$ is then

$$\hat{C} = \begin{bmatrix} Var(\hat{\eta}_1) & Cov(\hat{\eta}_1, \hat{\eta}_2) & \cdot \\ Cov(\hat{\eta}_2, \hat{\eta}_1) & Var(\hat{\eta}_2) & \cdot \\ \cdot & \cdot & \cdot \end{bmatrix}.$$

As shown by Card and Lemieux (1996), under the null hypothesis that the model is correctly specified, the goodness of fit statistics G is asymptotically chi-squared distributed with M - 3 degrees of freedom

$$G = \hat{\eta}' C^{-1} \hat{\eta}$$

The goodness of fit statistic in the jointly estimated models is always above 1% critical values. However, the models estimated separately for men and women fit considerably better. The single-skill model seems to explain the 1989 wages well, as the goodness of fit statistics are below the 5% critical values for both men and women. For women, the goodness of fit statistics are below the 5% critical values also in 1995 and close to it in 1983. Adding supply effects resulted to a considerable improvement for model fit for women in 1983 and 1989, when female participation in the labor force grew rapidly.

Table 4a. Single index model for cell means
Dependent variable mean log cell wage in 1983

	All			Male			Female		
Constant	1.018 (0.174)	0.809 (0.196)	-0.697 (0.424)	0.833 (0.366)	0.583 (0.446)	-0.432 (0.597)	1.173 (0.269)	0.359 (0.331)	-0.364 (0.564)
Mean cell wage in 1977	0.894 (0.019)	0.919 (0.022)	1.087 (0.054)	0.914 (0.040)	0.943 (0.049	1.062 (0.076)	0.877 (0.031)	0.971 (0.038)	1.063 (0.077)
Relative supply change 1977 - 83		-0.058 (0.029)	-0.035 (0.023)		-0.056 (0.058)	-0.005 (0.036)		-0.098 (0.031)	-0.076 (0.030)
Mean years of education in cell			-0.017 (0.006)			-0.018 (0.008)			-0.011 (0.008)
Mean age in cell			0.002 0.001			0.003 (0.001)			0.001 (0.001)
Female			0.066 (0.019)						
N	48	48	48	24	24	24	24	24	24
R squared	0.979	0.981	0.994	0.960	0.962	0.989	0.974	0.986	0.995
Goodness of fit (deg. of freedom)	128.5 (46)	113.3 (45)	64.2 (42)	81.7 (22)	75.5 (21)	35.9 (19)	43.7 (22)	33.0 (21)	22.3 (19)

Table 4a cont. Dependent variable mean log cell wage in 1989

	All			Male			Female		
Constant	0.720 (0.097)	0.620 (0.108)	-0.519 (0.201)	0.448 (0.127)	0.206 (0.137)	-0.160 (0.352)	0.502 (0.267)	0.070 (0.321)	-0.513 (0.762)
Mean cell wage in 1983	0.939 (0.011)	0.950 (0.012)	1.087 (0.025)	0.968 (0.014)	0.994 (0.015)	1.042 (0.044)	0.964 (0.030)	1.013 (0.036)	1.093 (0.098)
Relative supply change 1983 – 89		-0.023 (0.012)	-0.044 (0.007)		-0.042 (0.015)	-0.046 (0.015)		-0.045 (0.022)	-0.042 (0.023)
Mean years of education in cell			-0.007 (0.002)			-0.004 (0.004)			-0.007 (0.008)
Mean age in cell			-0.001 (0.001)			0.001 (0.001)			-0.001 (0.001)
Female			0.061 (0.008)						
N	48	48	48	24	24	24	24	24	24
R squared	0.994	0.995	0.999	0.996	0.997	0.998	0.978	0.984	0.987
Goodness of fit (deg. of freedom)	74.1 (46)	72.9 (45)	64.6 (42)	31.5 (22)	30.9 (21)	30.5 (19)	36.3 (22)	31.9 (21)	29.0 (19)

Dependent variable mean log cell wage in 1995

	All			Male			Female		
Constant	1.183 (0.186)	1.237 (0.214)	0.634 (0.682)	1.125 (0.359)	1.269 (0.426)	0.486 (1.081)	1.075 (0.241)	1.023 (0.300)	1.066 (0.563)
Mean cell wage in 1989	0.873 (0.020)	0.867 (0.023)	0.926 (0.085)	0.879 (0.038)	0.864 (0.045)	0.945 (0.149)	0.886 (0.027)	0.891 (0.033)	0.869 (0.073)
Relative supply change 1989 – 95		0.012 (0.022)	-0.031 (0.022)		0.032 (0.049)	-0.032 (0.043)		-0.006 (0.020)	-0.045 (0.016)
Mean years of education in cell			-0.005 (0.008)			0.010 (0.014)			0.003 (0.007)
Mean age in cell			0.003 (0.001)			0.003 (0.002)			0.003 (0.001)
Female			0.016 (0.026)						
N	48	48	48	24	24	24	24	24	24
R squared	0.977	0.977	0.989	0.961	0.962	0.983	0.981	0.981	0.995
Goodness of fit (deg. of freedom)	95.4 (46)	95.4 (45)	61.7 (42)	62.5 (22)	60.9 (21)	34.9 (19)	32.3 (22)	32.1 (21)	23.2 (19)

Note: Estimation method is measurement error corrected least squares. The goodness of fit tests the null hypothesis of perfect fit, ie. according the null the residual term is entirely sampling error.

In Table 4b, the single-index model is estimated for the 10th, 50th and 90th percentiles[11] of the cell wage distribution. These percentiles were regressed on the corresponding wage percentiles in the previous survey, quantile-specific constants and relative supply changes. The results confirm the results from the means data. Coefficients of the previous period wages are in all but three cases below one. The difference is significant in all models from the joint data and, in 1995, also in the separately estimated models. For separately estimated models in 1983 and 1989, the coefficient of the previous period wage is never significantly different from one, as long as the supply effects are included in the model. The estimated supply effects are quite similar to the previous estimates, though insignificant in 1995.

The model predicts that the different quantiles have common slopes and separate intercepts. According to the equation (8), if the return to skills has increased (the coefficient of the previous period wage is greater than one), the intercept of the lower quantiles should have a positive sign and the intercept of the higher quantiles a negative sign that is approximately equal in absolute value. If returns to skills decreased, these signs should reverse. This prediction is roughly consistent with the data in cases where the coefficient of the previous period wage is significantly different from one. On the other hand, the common slopes prediction is not supported by the data. The common slopes assumption is rejected at 1% significance level in 12 out of 15 cases where the model with separate slopes could be estimated.[12] Also, the goodness of fit statistics indicate specification problems. The goodness of fit statistics are above 5% critical values in all estimated equations and in all but four cases also above 1% critical values.

[11]Qualitatively similar results were obtained fitting the model to the 25th, 50th and 75th percentiles. According to the goodness of fit statistics the model fits slightly better to the quartiles, which may indicate that the normal approximation used in calculating sampling errors performs worse in the tails of the distribution.

[12]In three cases adding interactions of the quantile dummies and the previous period wage resulted to a noninvertable (measurement error corrected) moment matrix.

Table 4b. Single index model for wage quantiles

Dependent variable 10th 50th or 90th percentile of log wage distribution in 1983

	All		Male		Female	
Constant	0.894 (0.115)	0.608 (0.125)	0.690 (0.212)	0.253 (0.237)	0.845 (0.208)	-0.274 (0.270)
Corresponding wage percentile in 1977	0.908 (0.013)	0.941 (0.014)	0.930 (0.023)	0.980 (0.026)	0.914 (0.024)	1.043 (0.031)
Relative supply change 1977 – 83		-0.080 (0.019)		-0.100 (0.031)		-0.129 (0.025)
Dummy for 10th percentile	-0.014 (0.010)	-0.003 (0.009)	-0.018 (0.015)	-0.001 (0.015)	0.002 (0.013)	0.047 (0.013)
Dummy for 90th percentile	0.020 (0.009)	0.009 (0.009)	0.019 (0.016)	0.002 (0.015)	0.010 0.012)	-0.030 (0.012)
N	144	144	72	72	72	72
R squared	0.988	0.990	0.985	0.988	0.987	0.993
Goodness of fit (deg. of freedom)	285.7 (140)	257.4 (139)	151.7 (68)	136.4 (67)	126.4 (68)	108.2 (67)
F-test for quantile specific slopes (p-value)	7.59 (0.001)	8.55 (0.000)	1.27 (0.289)	1.62 (0.207)	6.29 (0.003)	18.0 (0.000)

Dependent variable 10th 50th or 90th percentile of log wage distribution in 1989

	All		Male		Female	
Constant	0.600 (0.082)	0.483 (0.093)	0.355 (0.088)	0.114 (0.099)	0.257 (0.210)	-0.332 (0.255)
Corresponding wage percentile in 1983	0.951 (0.009)	0.964 (0.010)	0.977 (0.010)	1.004 (0.011)	0.991 (0.024)	1.057 (0.029)
Relative supply change 1983 – 89		-0.027 (0.011)		-0.043 (0.011)		-0.060 (0.017)
Dummy for 10th percentile	-0.024 (0.006)	-0.020 (0.006)	-0.025 (0.006)	-0.016 (0.006)	-0.001 (0.012)	0.020 (0.012)
Dummy for 90th percentile	0.057 (0.006)	0.053 (0.006)	0.051 (0.006)	0.041 (0.006)	0.042 (0.011)	0.023 (0.012)
N	144	144	72	72	72	72
R squared	0.995	0.995	0.998	0.998	0.991	0.993
Goodness of fit (deg. of freedom)	221.7 (140)	220.9 (139)	92.3 (68)	92.9 (67)	118.4 (68)	112.5 (67)
F-test for quantile specific slopes (p-value)	562.5 (0.000)	na	0.8 (0.441)	Na	129.2 (0.000)	Na

Table 4b cont.
Dependent variable 10th 50th or 90th percentile of log wage distribution in 1995

	All		Male		Female	
Constant	1.202 (0.146)	1.156 (0.169)	1.103 (0.248)	1.248 (0.294)	1.060 (0.235)	0.628 (0.280)
Corresponding wage percentile in 1989	0.871 (0.016)	0.876 (0.018)	0.882 (0.026)	0.866 (0.031)	0.886 (0.025)	0.934 (0.031)
Relative supply change 1989 – 95		-0.010 (0.017)		0.032 (0.033)		-0.051 (0.019)
Dummy for 10^{th} percentile	-0.025 (0.012)	-0.023 (0.123)	-0.044 (0.019)	-0.050 (0.020)	0.004 (0.015)	0.019 (0.015)
Dummy for 90^{th} percentile	0.043 (0.012)	0.041 (0.012)	0.033 (0.019)	0.039 (0.021)	0.043 (0.015)	0.027 (0.015)
N	144	144	72	72	72	72
R squared	0.982	0.982	0.981	0.981	0.982	0.984
Goodness of fit (deg. of freedom)	246.5 (140)	245.4 (139)	144.4 (68)	140.7 (67)	95.7 (68)	91.9 (67)
F-test for quantile specific slopes (p-value)	24.1 (0.000)	24.5 (0.000)	26.3 (0.000)	24.3 (0.000)	6.36 (0.003)	8.35 (0.001)

Note: Estimation method is measurement error corrected least squares. The goodness of fit tests the null hypothesis of perfect fit, ie. according the null the residual term is entirely sampling error.

Overall the single-index model, augmented with supply effects, explains changes in the mean cell wages reasonably well but does not adequately describe changes in the within-cell variation. It appears that our supply-demand framework provides a sufficient explanation for changes in the between-group variation, but something else is going on within the skill groups. The next section presents some conjectures of other possible forces. It should not be surprising that attention is now focused on the effect of unions.

5.4.2 Conjectures on the intervening mechanisms: Institutions do matter.

Up to this point not much has been said about the unions or the centralized wage bargaining process and its impact on the wage structure. Gottschalk and Smeeding (1997) note that the correlation between the degree of centralization and the trend in inequality is clearly negative. The countries with the least centralized labor markets (UK and US) have experienced the largest increases in inequality. Still, it would be difficult to argue that the unions were immune to market pressures and could set wages exogenously without considering supply and demand conditions. Therefore, treating the wage bargaining mechanism as an exogenous factor could be quite misleading.

Institutions do have a crucial role in wage setting. The wage bargaining system is highly centralized in Finland. Since 1969 wage negotiations have been co-ordinated on the national level and annual wage contracts covered all unions. Union wage agreements are also binding for the non-union workers. In order to provide an explanation for the change in inequality, there should have been a change in the wage setting system. However, during the period under the study, no major changes occurred. Membership in the labor unions actually grew during the 1980's, especially among the white-collar workers (Kyntäjä, 1993). Yet, there has been some variation in the degree of centralization from year to year. Between 1977 and 1995 there were five occasions when no central agreement was achieved and wages were negotiated at the industry level. Also, even in years when a national central agreement was negotiated not all unions accepted the contract.

Ruutu (1997) has calculated the share of the union members that were outside the central bargain in each round of the wage negotiations. This share shows a strong correlation with the increase in wage dispersion. Figure 6 shows the change in the 90/10 wage differential in three year intervals[13] with the average fraction of union members outside the central bargains that were in force during the same periods. With just six observations, the statistical significance is questionable but the visual impression is striking. Of course, this association does not imply a causal relationship. The changes in inequality observed in Figure 6 could also fairly well match other time series, such as the growth rate of the GNP. In fact, Ruutu (1997) shows that the unions are more likely to opt out from the central bargain if the unemployment has been low, or the economic growth has been high in the year before the wage negotiations.

[13]Inequality measures were not available for the years -78, -79, -81, -82, -84 and -85. Focusing on three year intervals skips these years.

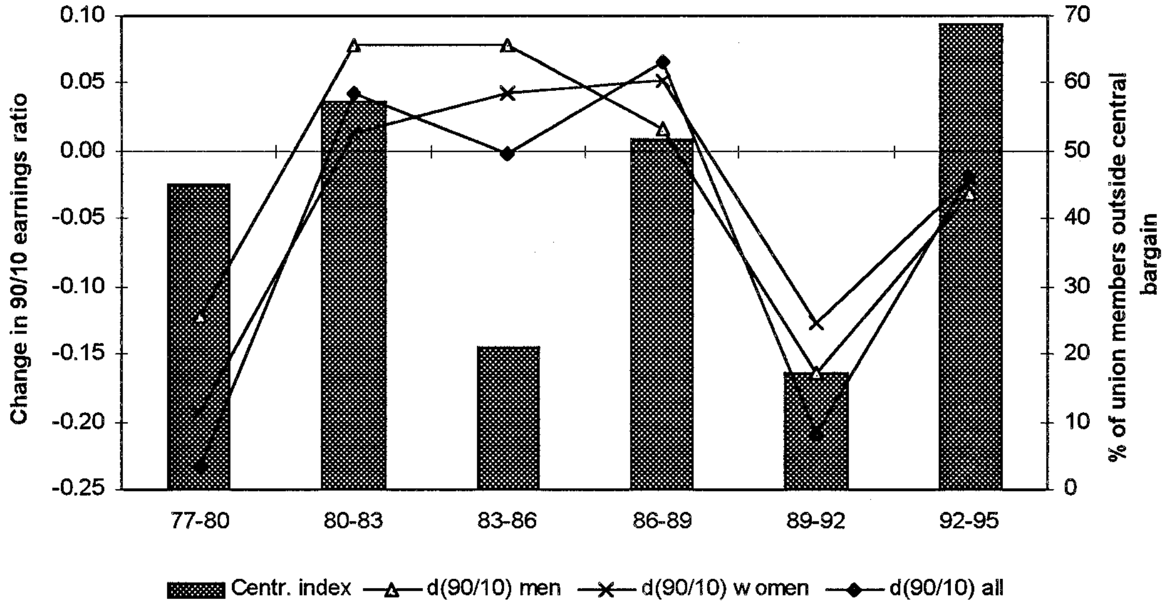

Figure 6. Changes in aggregate wage dispersion and centralization of wage bargaining.

Note: Centralization index (vertical bars) is a three year average of the percentage of all union member who were outside central agreement. It includes members of SAK unions which did not accept the centrally negotiated bargain and members of other central union organizations (STTK, AKAVA) in the years when more than one central bargain was negotiated. Source: Ruutu (1997). Inequality indices (thin lines) were calculated from OECD data shown in Figure 2 and refer to the three year change in the decile ratios.

Bearing in mind qualifications about the direction of causality, the results still suggest that the changes in the bargaining system may provide the missing piece for understanding the changes in the wage distribution. The growing within-group wage dispersion in the end of the 1980's might well be related to the decentralization, as well as the supply and demand factors. The contraction of the wage distribution in the early 1990's occurred during four successive rounds of nationwide wage negotiations. After industry level bargains in 1994 and 1995 wage differences increased again.

5.5 Concluding comments

In this paper, a simple supply and demand framework is developed in order to explain the observed changes in the aggregate wage distribution in Finland from 1977 to 1995. A single-skill model, augmented with supply effects, fits relatively well the data on age/education cell means, but less well on quantiles. Therefore, it seems that the changes in age and education related wage differentials can be reasonably explained with just supply and demand factors, but changes within groups of similar age and education requires more. One possible explanation is based on changes in the institutional setting.

Comparing different time periods with goodness of fit indices reveals that the augmented single-skill model performs best in explaining changes in the period from 1983 to 1989. Changes in the other periods are not as well accounted for. The changes in the wage distribution during the rapidly increasing unemployment in the 1990's would be particularly interesting, but its closer examination is outside the scope of this paper. Also, it is clear that the single-index model cannot account for a relative increase in women's wages. The most obvious explanation for the increase is the shift in the female skill distribution caused by increasing labor force participation and increasing amount of work experience.

Most interestingly, very little evidence of increasing demand for skills is found in this study. At least changes in the demand for skills do not show as widening skill related wage differences. Consequences of such apparently rigid wage structure for relative employment of different skill groups, particularly during high unemployment, will be an interesting topic for further research.

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Appendix 1. Cross - section regressions

	All				Male				Female			
	1977	1983	1989	1995	1977	1983	1989	1995	1977	1983	1989	1995
Education												
Lower secondary	0.090	0.076	0.069	0.051	0.100	0.092	0.090	0.064	0.072	0.057	0.043	0.027
(11 years)	(0.004)	(0.007)	(0.008)	(0.010)	(0.005)	(0.009)	(0.012)	(0.015)	(0.006)	(0.009)	(0.011)	(0.013)
Upper secondary	0.250	0.228	0.218	0.183	0.283	0.261	0.248	0.208	0.201	0.177	0.179	0.144
(12 years)	(0.005)	(0.009)	(0.010)	(0.011)	(0.007)	(0.013)	(0.015)	(0.017)	(0.007)	(0.012)	(0.013)	(0.015)
Higher educ..	0.396	0.337	0.388	0.364	0.451	0.387	0.418	0.395	0.337	0.283	0.349	0.313
lowest level	(0.008)	(0.012)	(0.014)	(0.014)	(0.012)	(0.020)	(0.022)	(0.023)	(0.010)	(0.016)	(0.017)	(0.019)
Undergraduate.	0.515	0.516	0.503	0.366	0.564	0.550	0.593	0.313	0.465	0.483	0.444	0.387
(BA level)	(0.013)	(0.019)	(0.019)	(0.023)	(0.021)	(0.028)	(0.037)	(0.042)	(0.015)	(0.025)	(0.020)	(0.024)
Graduate.	0.697	0.637	0.646	0.594	0.724	0.661	0.664	0.611	0.662	0.601	0.632	0.564
(MA level)	(0.011)	(0.016)	(0.017)	(0.015)	(0.014)	(0.021)	(0.021)	(0.021)	(0.018)	(0.025)	(0.027)	(0.020)
Postgraduate level	0.770	0.781	0.751	0.685	0.793	0.800	0.754	0.696	0.749	0.712	0.805	0.685
(Licentiate. PhD)	(0.033)	(0.061)	(0.057)	(0.046)	(0.034)	(0.070)	(0.064)	(0.061)	(0.099)	(0.075)	(0.096)	(0.066)
Age	0.048	0.056	0.050	0.050	0.056	0.067	0.059	0.054	0.039	0.045	0.043	0.045
	(0.001)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)	(0.004)	(0.001)	(0.003)	(0.003)	(0.004)
Age squared	-0.001	-0.001	-0.001	-0.000	-0.001	-0.001	-0.001	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Female	-0.333	-0.290	-0.252	-0.219								
	(0.003)	(0.006)	(0.007)	(0.008)								
N	34671	12711	11800	7667	18483	6799	6062	3882	16188	5912	5738	3785
R squared	0.486	0.481	0.444	0.448	0.414	0.445	0.400	0.420	0.338	0.343	0.370	0.390
RMSE	0.287	0.283	0.301	0.276	0.298	0.295	0.323	0.298	0.271	0.264	0.274	0.248

Note: The dependent variable is log monthly wage calculated from annual pre-tax wage and salary earnings and months worked. Part-time months are counted as half a months. Data include wage earners who are between 20 and 64 years old and have worked at least six full-time equivalent months. Observations with wage and salary index adjusted monthly earnings less than 3000 mk in 1995 prices are excluded. This exclusion removed only 8 observations in 1995, but more in earlier years, probably due to less reliable data on months. All columns include 12 regional dummies and 9 industry dummies. Omitted category in schooling dummies is the group with only compulsory education. Sampling weights used in estimation, and standard errors are corrected for within PSU (family) correlation and stratification.